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THE HUXLEY LECTURE ON CLINICAL SCIENCE WITHIN THE UNIVERSITY*

BY

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I

In response to the invitation with which you have honoured me, I am going to plead before you to-day that the biological science in which I am interested should be given fuller university status. It is a special privilege to be allowed to bring these views to a university closely engaged in considering problems of medical education, and in a lecture commemorating a man universally recognized as a chief pioneer both in science and in education. But though Thomas Huxley gave his brilliant mind to the search for and diffusion of knowledge in physiology and in other branches of biological science, his interest was not directed to studies of human disease. For this reason you will perhaps permit me to turn for a moment to his distinguished contemporary, James Paget, who, at first similarly attracted by biology, was subsequently drawn on to become a noted investigator of disease in man and a most gifted surgeon. It was the development of Paget in this direction which enabled him, while very conversant with contemporary biological and physiological thought and fully alive to its importance, to make these deliberate and weighty statements in speaking of clinical work:

"I feel sure that clinical science has as good a claim to the name and rights and self-subsistence of a science as any other department of biology; and that in it are the safest and best means of increasing the knowledge of diseases and their treatment. . . . Receiving thankfully all the help that physiology or chemistry or any other sciences more advanced than our own can give us, and pursuing all our studies with the precision and circumspection that we may best learn from them, let us still hold that, within our range of study, that alone is true which is proved clinically, and that which is clinically proved needs no other evidence."

I use these clear and memorable sentences of sixty-five years ago, with which I find myself thinking in such close accord to-day, to introduce the matter of the present address. In using the term "clinical science" Paget thought exclusively of researches upon living men. I shall use this same term to convey his meaning, but shall widen it, and define clinical science as the branch of knowledge that centres upon diseased human beings, but which also includes directly relevant parts of the allied sciences. The central field of work is the human patient; and this will remain the field peculiar to the science. But work will often extend to more distant boundaries; these will include human physiology, because the normal is the control to the diseased man; will include the morbid anatomy and the bacteriology of man; and will include "experimental medicine," which has come especially to mean such experiment upon the lower animals as is inspired by clinical problems.

I am putting before you the idea that clinical science *so defined* should find its place and acquire proper status within the university. It has sometimes been pointed out that what I have spoken of as clinical science is not clinical medicine and clinical surgery as these are practised, and that certain of its activities might justly be termed physiology or pathology. I know that this is so. The distinction between the science and the art of medicine is intentional; they should be kept as separate ideas, and regarded as separate directions of human endeavour, in working to practical ends; though it is to be said that there is no wish to infer that the science and the art can never be pursued by one person. By intention, too, distant boundaries have been fixed. To give independent strength to the science it must be unhampered in its movements. The boundaries between individual sciences may not be maintained rigidly without hindering progress; overlaps between clinical science, pathology, and physiology are vital to medical science as a whole; they bind together three activities, which in process of time have grown too far apart from one another. We are here engaged in discussing a serious practical question—namely, the relation of the university to branches of knowledge that concern the health of mankind. The university cannot successfully enter this field of activity without setting up within its own domain a strong scientific department inspired by direct clinical interests.

In attempting to picture clinical science within the university, the relation and attitude of the several medical sciences to each other obtrude themselves. In recent times discussion has sometimes turned from mere statements of achievement, possible and real, to a comparison of the merits of these sciences, or to a comparison of the relative importance of the contributions from ward and laboratory. These two comparisons are of course distinct, for in many of its activities clinical science is to-day essentially a laboratory science. I understand by a laboratory a place well equipped with apparatus and other facilities for working by special and quantitative methods; I cannot accept the common notion that a laboratory is a place to which animals but no patients are admitted. It is proper and necessary periodically to describe and emphasize the mass of knowledge that mankind undoubtedly derives both from the physiological laboratory and from the clinical ward with its associated laboratories. But an attempt to assess the relative merits of these different activities swings discussion from useful into controversial channels, and simultaneously diverts attention from central truths—namely, that the merits of these various forms of work are not in fact comparable, and that both clinical science and physiology, both ward

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work and laboratory work, are absolutely and therefore equally essential to the attainment of those practical goals towards which all groups of workers in these fields are striving. The sciences considered form a closely interconnected series, which should be encouraged to grow as one harmonious whole. To the more applied science, any growth in the purer science that nourishes it is a gathering of its own ultimate strength ; and any growth in the applied science promises to the work of the purer science a surer fruition. The welfare of each member of the series is highly important to all other members ; the general strength will largely find its measure in that of the weakest component. Here, therefore, is the strongest motive for mutual good will and support. Nothing will more quickly bring the allied medical sciences into close understanding and sympathy, which from every standpoint is so desirable, than that they should be placed upon just the same footing within our universities ; it is essential to this purpose that each should be founded on the cardinal principles, which shape sciences universally, and that each should possess scope sufficing to give independent strength.

II

It is necessary to my main theme to show that clinical science has the power to contribute abundantly to progress, and this leads me on to speak briefly of the broad relations of this science to problems of human disease. Studies of disease cannot begin until disease is recognized and in some measure understood ; real knowledge of disease is derived only from disease itself, it is not built up in the form of concepts of perverted function. No human disease is known to be curable until man has been cured of it. It is in the very nature of things that, in its initiation and in its culmination, work relating to human disease must be done on man himself. Once the problem has been formulated, and before the final work on man is done, in a middle phase, study may become diverted to the lower animals. Such work, provided that it still concentrates upon the problem of disease, is still within the province of clinical science, as this has been defined, though, be it granted, it would fall without under a narrower definition. But under one or other definition the facts here briefly stated bring us to comprehend that the opportunities of clinical science in guiding and co-ordinating work as a whole are unique.

Notable advances in the practice of medicine and surgery come chiefly out of work concentrating upon the main problem studied in its natural environment, rather than from researches of a more remote and recondite kind. These latter are not to be placed upon one side as unimportant, or even as less important, to progress: but their influence is less immediate ; they provide us with methods of precision ; they suggest to us tests to employ and experiments to undertake ; they help us much in explaining and appreciating the manifestations of disease and the mode of action of our remedies. But the management of disease is rarely to be deduced from first principles ; it is the direct approach that attains swift success. The value of facing the problem directly has been illustrated repeatedly. When Pasteur was called to investigate fermentation in wine, the disease " pébrine " in silkworms, and the disease anthrax in sheep, he dealt with each of these problems in its stronghold ; starting from the disturbance or the disease itself, he concentrated upon it and ultimately and in each case himself brilliantly won the remedy. Jenner similarly dealt with small-pox, seizing during his clinical experience of the disease upon folklore, from which he worked on and subdued a scourge; in like fashion and starting from compound fractures Lister attacked and swept away the sepsis of surgery. It is not often that the efforts of one man can achieve so much ; but the same form of successful attack can be illustrated by the work of Ross upon malaria, of Mackenzie upon auricular fibrillation, and by the recent work of Laidlaw and his colleagues upon the distemper of dogs. A more involved illustration is that of the discovery of insulin and its effect in diabetes melitus. First comes the recognition of the disease, then recondite laboratory research happens upon a new effect of excision of the pancreas, both essential steps ; but the present aptness of the illustration lies in its final stages. Banting, a young doctor familiar with the malady diabetes, was inspired by learning from a clinical paper that obstruction of the duct of the pancreas leads to atrophy of the excretory parts of the gland ; out of this idea he directly devised the method that first proved the presence of an internal secretion, insulin, in the remainder of the gland, and from this Banting and his colleagues themselves drove on to the end-point, the successful administration of insulin to patients.

These illustrations, by showing how important to the result is concentrated attack upon a problem in its own environment, place clinical science and the more recondite sciences in their true perspective. I pass on to consider some ways in which clinical science works.

Scientific discovery begins with observation, and it will clearly be perceived that discovery relating to disease forms no exception, but will always start from observations upon the disease. On previous occasions I have emphasized the importance of new descriptions of states of disease, and the associated attempts to define them precisely, knowing how vital is this branch of our work to progressive study of disease. Those who come into intimate contact with clinical researches are soon forced to recognize the profound influence of terms on progress, and the value of their accuracy. When we name disease, we invent terms which we realize to be abstractions. They represent abstract conceptions in precisely the same sense as do the terms atom, cell, vitamin ; and as terms they serve exactly similar purposes.

A physiologist begins an analysis, he isolates what he terms carbohydrate, fat, protein ; he attempts to study the origin, nature, and destination of things for which these terms are supposed to stand, and, as he studies, new qualities are found, he subdivides, and new names and new definitions spring up. So with our named diseases ; we set out to isolate and to bring sharpness to our image of states of disease and begin to inquire what they are and whence they come. The names of diseases are not immutable. As with the physiologist, so with us, our terms change and our definitions shift as we work ; such changes are inevitable, and constitute the adjustments, nay a measure, of progress. Our terms stand to us for conceptions which may be relatively vague or relatively precise, but which serve for intercommunication, and are numbered, like working hypotheses, among indispensable tools. Our definitions are beams of thought focusing in terms, and this focusing and sharpening of focus represents a high form of intellectual endeavour. But these mental activities, conveniently summarized in terms and in definitions, depend upon the close and endless work of observation and experiment, upon inquiries into the manifestations of disease and into its origin and natural history. This kind of work is little understood outside the ranks of actual workers in this field. There are the early attempts to isolate and separate states of disease in patients upon the simple basis of observation in the living. The resulting subdivision is crude, and constitutes no more than a beginning. There follow the more intense refining processes ; hypothetically separate states of disease are closely scrutinized, underlying morbid lesions are examined, the various manifestations of the disease are subjected to

analysis, the cause of the disease is explored, and found perhaps in a change of environment or in the introduction or growth of organisms within the body. In these studies especially, knowledge and method derived from the allied sciences are used, in these studies the observational method begins to be found inadequate and the experimental method comes into play. The studies are intricate and often highly technical, and, as they proceed and the nature of the malady and its natural history are explored, ideas alter, and we begin to define and to rename. Thus, while the early names base themselves on simple observation, the later names and definitions base themselves on far more elaborate and difficult studies. The simple process of observation, from the nature of the case, soon tends to exhaust itself, but the accurate defining of states of disease does not. Here is an ever present opportunity of clinical science. The defining of states of disease, as it has been outlined, is fundamental to and inextricably interwoven with the study of these diseases ; I affirm that this defining is yet in its infancy, that its primitive state is inhibiting the progress of medical science, but that as this branch of knowledge develops, our conceptions of states of disease will change profoundly.

There is more to be said about experiment ; for in this direction our point of view in studying disease has begun to alter rapidly in recent years. It should be manifest that whenever we interfere with a patient, as when we employ any test of a patient's reaction to something introduced from without, to exercise, to a foodstuff or to any other substance, or to any physical stimulus, we experiment. Experiment has long been applied in the sphere of treatment. Every remedy employed to-day is the result of experimentation on man, every new remedy we shall employ in the future must inevitably pass through this experimental stage. The method has made three incomparable gifts to medical science and the welfare of mankind—namely, vaccination, antiseptic surgery, and general anaesthesia. Experiments judiciously selected and properly conducted are not only harmless, they are often or usually beneficial, teaching us more rationally to regard and manage the patient's condition. Those who have charge of patients are in this respect loaded with a grave responsibility ; only harmless or beneficial experiments are justifiable. It must be understood that responsibility cannot be evaded by refusal to experiment ; experiments always have been conducted, always will be, for they are essential to progress. It is obviously very important that such experiments as are made should be made with the consent of the patients, should be made in a safe manner, and that they should be conducted so that they give reliable answers to the questions asked. Of experiment in medicine, and especially in the examination of remedies, much that is done lacks precision and proper control ; consequently, much of the work is valueless and must be repeated, and so very many more experiments are carried out than is necessary to determine the results. You will apprehend that, in so far as remedies are concerned, I am advocating a reduction in the numbers of experiments performed, and their conduction upon a stricter, safer, and more productive basis than hitherto. But the fruitful application of the experimental method to states of disease does not end with an investigation of remedies. There are many opportunities of testing patients by harmless interferences in such ways as to explain manifestations and causes of disease. In this direction the experimental method has only begun to bring us new knowledge. That is so because, apart from the search for remedies, more purely observational methods are traditionally natural to practitioners of medicine and surgery. The experimental method requires for its employment a special outlook and training ; thus, those having easy access to patients use this method and its controls little ; and those trained to employ it are often denied the access, which they desire. Those working in other fields are able to recognize directions in which the method has already achieved success ; but only those intimately in contact with the work of clinical science perceive the manifold directions in which it remains to be applied. I will not weary you by enumerating past results, or by pointing out specific problems now requiring this method of study, but will state categorically that it is peculiarly appropriate to innumerable manifestations of disease, and to many problems of causation. This statement is true not simply of one or two types of disease, but of problems connected with almost every variety of disease ; the method is of almost boundless application. Experiences of work in diverse directions convince me that experiment is as fundamental in investigating disease in patients as it is in the case of other biological sciences, and that clinical science properly established would soon demonstrate the fact abundantly.

This general survey of the particular activities of clinical science, activities which have been considered previously on more than one occasion, will suffice to show that this science deserves very full encouragement. Briefly, the starting-point of the practical problems of medicine is the patient ; it is clinical work alone which can formulate the problem, and it is the clinician who is best placed to guide it to a fitting termination ; but this directing of work requires wide knowledge and often intimate experience not only of clinical methods, but of the methods and facilities afforded by allied sciences. The experimental method, which I foresee must play an increasing part in future work, calls for a particular point of view acquired by long training and experience in suitable original work. The employment of intricate apparatus and intimate knowledge of tests of precision make deep inroads into reserves of energy and time. While we may still expect to see many valuable discoveries coming from men engaged in the practice of medicine and surgery, it is increasingly clear that in many directions advances would more readily come from men free to give their undivided energies and thoughts to the work of progress, to the exclusion of routine. It is for this reason that the establishment of posts of clinical research has seemed so necessary. Because men must be trained to do such work, because it is right that those professing such work should enjoy the stimulus derived from teaching and from an appropriate atmosphere, it is necessary for clinical science to become established within the walls of the university.

III

To discuss the important matter of educating medical students is not the object of this lecture ; but in ascertaining the place of clinical science within the university this problem necessarily comes under review. It will be common ground, whether knowledge to be conveyed refers to practical art or to science, that education is most vigorously conducted by close contact between the student and the master of his subject ; methods of work are learned from demonstration, methods of thought by conversation ; communion inspires. If you would learn to do a thing, you go to one who does it well ; you watch, you listen, and your own first attempts are made under supervision. The teaching of medical practice began and has continued until recent times under such a simple system of apprenticeship. The final stage of medical education in this country still rests solidly upon this traditional foundation, centring upon clinical demonstrations and upon a system of clerkships and dresserships ; the high value of this method has obtained wide recogni-

tion. The most fitted to teach practical medicine and surgery are those successfully and daily engaged in its practice. But in a simple apprenticeship system, where a doctor has but one or more pupils, the standard of teaching would vary too greatly and would be in the average too low ; therefore students are brought together under teachers chosen for their unusual energy and ability. While it is obvious that the best teacher for general practice is a first-rate general practitioner, such practitioners could rarely engage actively in teaching numbers of students and yet pursue their ordinary work. For the reason that among able doctors hospital physicians and surgeons are the most readily available, teaching has fallen into their hands. This has formed for many years a more or less satisfactory compromise, though one that becomes less so in the measure that consulting work grows more specialistic in its outlook. The main principle emerging is that the final stages in the preparation for practice should be conducted by practising physicians and surgeons. Efficient medical practitioners are not scientists ; the bulk of their daily work is not to create or to discover, but to follow the established rules of an art ; as in the case of other practical arts their work, while highly skilled and often ingenious, is nevertheless chiefly repetitive. Preliminary education in science I shall allude to later, for the moment it is the immediate preparation for practice that concerns us ; of this I would say, that as it is an art, it should be taught by those who practise it ; and we may be content that while such a system is basically sound, it happens also to be the most economical.

Enough has been said to enable me to bring the final stages of the education of the medical practitioner into proper perspective and to display more clearly my views of the main functions of the university in its relation to the final subject of the medical curriculum. In thinking of the education of the practitioner, one is forced to wonder if the final stages of his preparation for practice form an appropriate activity for the university. But this question of the extent to which it is proper for universities to prepare students along technical lines is one affecting so many other forms of vocational teaching that I shall not venture further to consider it. I shall assume for the purposes of present argument that universities will continue at least to direct vocational education. But if this assumption is a correct one it is necessary to come to two conclusions. On the one hand the university must be prepared to train, or to direct the training of, students in medicine as a practical art. On the other hand, it must take heed that this vocational training does not displace, does not prevent the full development of, the purely scientific pursuits of medicine within the university.

It would seem quite unsound from the university standpoint that the training of either practitioners or consultants should be regarded as a primary or direct function of a professor of medicine or of surgery. I do not mean that it may not be his function largely to control such training ; I do mean that the transference of any of that work of training medical practitioners, which has been undertaken so well in the past by practising physicians and surgeons, to the professor would be a fundamental mistake. This training is manifestly the work of practitioners ; it is very definitely not the work of professors in the full university sense.

The man who is fit to train the practitioner to begin his life work must be thoroughly familiar with everything that makes for successful practice, must himself be in practice, must not only understand disease and its management in its detail, but must be competent in managing sick people and their friends ; he must be familiar with the science, skilful and precise in the craft, and versatile and diplomatic in the art. But these varied accomplishments will not fit him for the professorial chair. The work of the professor as I view it, though from many points of view intimately related, is distinct from this. I ask the questions: Is medicine in its final aspects to be studied only by those engaged actively in its practice ; is there to be no study of general concepts and of method, such as would be in more obvious accord with the recognized standard of university departments of science? Are we to remain content that activities of the latter kind should be pursued within the walls of the university solely in the departments of anatomy, physiology, and pathology? To answer these questions affirmatively is to deny medicine real university status ; to answer them negatively is to accord that status.

To make clear what I have in mind I will attempt broadly to sketch professorial duties. The first duty of a university professor is the advancement of knowledge of his subject by research ; his second duty is to encourage and to disseminate learning. These are the accepted duties of professors in scientific departments in all universities worthy of the name ; there is no reason to view differently the duties of those who profess the science now under discussion. The professor should be chosen primarily for his distinction in his science, or for his promise of such distinction. I see no reason why distinction as a practising doctor should be regarded as a qualification, but reasons against. There is no reason why the professor should not be appointed, as often are his colleagues in other scientific departments, as a young man. He should have been engaged actively in research in an appropriate field—namely, work that is actually undertaken upon patients, or laboratory work that is directly related to, and inspired by, problems arising in the ward or out-patient department. Such work he should continue, and he should supplement it by personally directing the similar activities of his pupils. His own research work will probably develop, as prolific work nearly always does, along special lines, but a broad outlook will be maintained by his contacts with the work of colleagues and assistants, and by his teaching.

His teaching should have breadth, and in its outlines should be accessible to all. I have already indicated strong dissent from the idea that it should be concerned primarily with the needs of practice. It should deal chiefly with the principles of the science, and with patients only to exemplify specific points.

The professor should study and expound the history of the medical sciences, and especially of clinical science, and this should be done, not from the biographical standpoint, but with the definite motive of determining the ways in which problems are solved. His business is not with the eccentricities of medieval quacks, but with the origins and development of ideas in the minds of chief contributors to medical knowledge ; and here, because true and full information is more readily available, his search should relate mainly to recent rather than to remote discoveries. He will trace the growth of knowledge, and will examine the influences of those systems or schools of thought which have prevailed. All this he will do in the attempt to discover sources of knowledge, prolific and sound methods of work.

He should deal with the problem of the cause of disease, or the conditions engendering disease, in its very many aspects ; his survey will cover very wide fields ; he will discuss the principles of hereditary transmission as these are known to relate to human disease, and he will inquire into the broad questions of the reaction of man to his environment ; thus he will discuss on general lines such problems as the meaning and effects of infection, the significance of sexual, racial, and other predispositions to disease, and the influence of one state of ill-health upon another, the reaction of human tissues

to physical injuries and to simple chemical poisons, the meaning of curious susceptibilities to more complex poisons, and the manner in which new growths arise in man.

He will attempt to analyse outstanding manifestations of disease, such objective phenomena as arise out of altered metabolism, changed temperature regulation, and water balance, and such subjective phenomena as pain, breathlessness, exhaustion, and the like. I have in mind the consideration of these and similar manifestations both upon broad lines and in detail, but always with special reference to human disease. There are innumerable matters of this kind for profitable discussion and work; they should not be relegated to the pathological laboratory or theatre, for this procedure breaks contact with the human material, which can be used so aptly to illustrate, which inspires related laboratory problems, and itself so often constitutes the most prolific focus of inquiry.

In regard to diagnosis, prognosis, and treatment let these be dealt with again on general lines, by the laying down of guiding principles, rather than by the systematic consideration of specific diseases. It would be well to ensure first of all that what is meant by diagnosis should be understood and to contrast it with new descriptions of states of diseases; to explore error in diagnosis and its practical significance, and to emphasize the need for its complete avoidance in research that depends on diagnosis; and to compare the empirical method of diagnosis with that based on understanding. It would be well to show the proper basis upon which prognosis is built up, and to discuss the relative values of theoretical and empirical methods of procedure. It would be well to explore the ways in which new remedies are discovered; to show how the curative properties of remedies or their power to alleviate are to be ascertained; and to teach how, by research, the use of known remedies may be rationalized, the remedies perfected, and the field of their efficacy widened.

In all these directions the professor should strive to formulate and inculcate the principles underlying methods of work and thought in his science; he should insist upon the constant need of accuracy in observation, and of controlled experiment; he should teach the student to appreciate the value of evidence, and encourage logical forms of argument and of sequential reasoning. He should stimulate inquiry. He should display the details of his own researches and those of others, and illustrate discovery and the principles laid down, by repeating chosen observations and experiments in the form of demonstrations.

There is no branch of medical science that offers more opportunity for demonstrations, there is none which offers more occasion for the use of reasoned argument, there is none which calls for higher intellectual attainment; for there is none which combines so many forms of scientific activity, and none in which the worker is forced so closely and so frequently to consider both simple and complex problems of cause and effect. It is in his physiological studies that the medical student is first called upon to stretch those intellectual faculties which are particularly used by scientists; for this reason physiology is rightly recognized to possess great value as a mental discipline. But this beginning does not suffice. These faculties should be stretched farther and kept under tension. Not only has clinical science the fullest competence to effect this development, but, so far as medical men are concerned, it is manifestly the most appropriate branch of science in which to bring these mental faculties to their highest efficiency; for it is the branch of science with which medical men will remain in closest contact, or that in which they will actually engage, not for a few years, but for the rest of their lives.

It is not for me to present you with a syllabus, but it has been incumbent upon me to show that there is ample scope in the shape of research, lectures, and demonstrations within the field of clinical science, for professorial endeavour, apart altogether from a systematic consideration of the diagnosis and treatment of diseases. These activities, supplemented by the work of guiding others and efforts to maintain firm links with the allied sciences, are enough, and with well-defined facilities can be brought to successful accomplishment.

The first requirement of a university department of clinical science is an out-patient and in-patient service sufficient to provide ample material both for the research and the teaching in the department. There is room for difference of opinion as to how large such departments should be, especially as to how many beds are necessary; this will decide itself partly by the local conditions and by the particular activities of the professor and his assistants. The first point I would make is that the cases upon which work is being done should be under the full control of the worker; experience has shown that this policy is alone practicable. Productive clinical research usually comes from an intense study of carefully selected cases, rather than from less careful studies of larger numbers; certainly this is so in all but the preliminary stages of investigation; that is to say, once the problem has become defined. Consequently, though large numbers of beds under the control of the worker are not required, the professor and each of his research staff must have an adequate complement. I regard the ideal arrangement as one in which the worker, while fully controlling, and having full responsibility for, all cases under investigation, is further provided with a service of patients from which he can select, and that he should be relieved from responsibility in the management of these; such relief can be given in a number of ways, as by clinical assistants drawn from those concentrating upon practical medicine, or by the close linkage between professorial and other services. Some additional beds will be required to satisfy the needs of teaching; but the total number required is definitely less than ordinarily serves a teaching unit of practical medicine or surgery. Out-patient and in-patient service should balance; and the system adopted should be adjustable so that the professor or his assistant may be free to concentrate from time to time on cases in one or other service, or upon work on lower animals.

Adequate laboratory accommodation is a second requirement; here the need, in so far as space, fittings, and equipment are concerned, will be appropriate to the type of work, and will vary from time to time. It is important to the workers that these laboratories should be as close to the clinical services as possible; in part they may be directly attached to ward or out-patient department; sometimes it can be arranged that they will serve for the special and uninterrupted examination of either patient or animal.

IV

In considering the usual medical curriculum of to-day, and asking where in it clinical science is to play its part, I would start from the statement that this curriculum is already overloaded. So far as the majority of medical students is concerned it is overloaded by the preliminary sciences, including anatomy, physiology, and general pathology; so far as all students are concerned it is overloaded on the clinical side in directions presently to be indicated. My personal belief, founded upon over twenty years of constant teaching of medical students, is that most medical students derive less intellectually from their preliminary work than is often supposed. But I have also become convinced that while for the more scientifically minded student very full measures of physics,

chemistry, and physiology are invaluable, the final work of the hospitals, as now conducted, is far from satisfactory. It is unsafe for any man, whose training in science has started well, to accept, on an authoritative and not on an evidential basis, the mass of statement of fact and hypothesis that the final student is now called upon to accept. There is, too, the very grave objection that during several years out of those most important to a man's intellectual development he is occupied in amassing disconnected information to the exclusion of contemplation. It seems beyond reasonable denial that the full course of work, supplemented as this is by purely clinical appointments, if suited to the training of a practitioner, must be unsuited to the man who is to make science a career ; it is wrong that he should so long be denied the chance of engaging in, or of being at least in close contact with, original work.

I think it is agreed that there are grave defects in medical education to-day, but agreement as to the remedy would come nearer if it were recognized that the right methods of educating practitioner and scientist are not the same. Cautious exploration may be necessary. But there is the principle to guide us—namely, a measure of separate consideration for two classes of student. I suggest as a basis of discussion the following plan in broad outline, and to simplify I shall omit considerations of those sciences which by consent form part of the basis of a general education.

There should be a first course consisting of outlines of human anatomy and physiology, both taught to ensure a clear understanding of the main mechanisms of the body and with special stress upon the needs of the final qualifying courses of instruction—that is to say, it should be particularly emphatic in teaching what is immediately applicable to the study of disease ; for such matter will always command particular interest, and will be the more willingly and more easily remembered. It is not suggested that this course should consist exclusively of such material, for much must also be taught solely to illustrate the principles of the sciences concerned and their methods of work.

The second course should be one of more advanced physiology, suited to a science degree and to those who intend to take up physiology, pathology, or clinical science as a career. It should not be compulsory for medical students ; it would ordinarily be taken in direct sequence to the first course, in some cases it would culminate in an honours degree, in some instances it might be delayed.

The third course should be the final or qualifying course, and it should consist on the one hand of general and practical instruction along present lines. For the qualifying examination it is necessary to demand a general standard of professional knowledge fitting the graduate to begin independent management of patients ; but present-day teaching must be simplified in all its branches. What is essential to general practice should be retained and emphasized, but there should be ruthless pruning away of what is insecure, intricate, or redundant, and of what belongs peculiarly to the province of specialism. Teaching should mainly concern itself with common maladies and with remedies of proved worth and ready application. There should be a determined effort to arrange facts and ideas in thoughtful sequence, and to train students to think logically as well as to observe keenly. This simplified final course should include the outlines of clinical science, and, thus arranged, should be the regular course for all medical students. Higher examinations should, I think, definitely differentiate between two classes of students. The Royal Colleges and perhaps the universities would still hold advanced examinations primarily testing professional abilities. But there is room for a university degree in medical science, which should not include medicine, surgery, or other branch of medical practice as such, but should centre upon disease, as this is studied in human beings ; and this degree should be intended to mark those contemplating an academic career.

It is my conclusion that it is largely within the power of our universities to establish the branch of work that studies disease in living people as a science, by removing the obligation to engage in and teach the practical art, and by treating clinical science on precisely the same basis as the allied sciences, physiology and pathology, are treated, thus bringing all the work into real harmony. The maintenance of equilibrium among these three may then be left with confidence to the interplay of individual and general needs, and to the mutual sympathy that is natural in any corporation of enlightened people working to a common end.

LOCAL ANAESTHESIA IN GYNAECOLOGY

BY

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LONDON

The employment of local anaesthesia for gynaecological operations has never, in this country, attained to anything like the popularity it enjoys in so many Continental and American clinics. Various explanations have been suggested for this contrast, the most likely being differences in the general standard of inhalation anaesthesia, and the extreme sensitiveness of Englishwomen to any form of genital exposure. But whatever the reasons, the fact is established, and tends to discredit a method enjoying certain advantages which cannot be ignored. In order to appreciate these advantages it will be useful, as a preliminary, to notice the defects inherent in the other types of anaesthesia generally employed.

Disadvantages of the Non-local Anaesthetics

Inhalation Anaesthesia.—It should be remembered that in spite of the modern use of "safe" anaesthetics like ether and nitrous oxide, and of the increasing number of professional anaesthetists, a definite mortality must still be ascribed to inhalation anaesthesia alone. Greenhill[1] found that ether was the cause of death in 0.15 per cent. of 10,325 abdominal operations, while Bartlett and Simmons[2] state that 12.3 per cent. of the 148 deaths following gynaecological operations investigated by them arose from the same cause. In addition, certain post-operative, though non-fatal, complications may ensue. Bronchitis, pneumonia, and toxic hepatic changes are not uncommon, while acidosis and dehydration shock may result from prolonged administration. Lastly, the painful retching and vomiting which so often accompany returning consciousness remain one of the most distressing sequels of this form of anaesthesia.

Spinal Anaesthesia.—The substitution of spinal for inhalation anaesthesia was primarily designed to eliminate most of the disadvantages of the latter, but increasing experience has been disappointing in this respect. Its dangers and difficulties may be summarized as follows. (1) It is actually less safe than inhalation anaesthesia. In a large series of cases investigated by Bortone[3] there was one death for every 311 spinal as compared with one for every 1,628 general anaesthetics. Babcock[4] found seventy-five deaths in 215,000 spinal anaesthetics, and Rapaport[5] seven in 500. Cooke[6] and Bower[7] both state that there are more deaths in proportion from spinal than from general anaesthesia, while Meeker,[8] whose experience com-

mands respect, says that the former " has a mortality still too high to recommend it as a method of choice." (2) There is always some degree of uncertainty. Osteo-arthritis and deformities of the spine, the presence of physiological or pathological abdominal tumours, and other factors may prevent spinal puncture ; while the degree and level of the anaesthesia cannot always be guaranteed, even in expert hands. A survey of the literature published to date reveals that spinal anaesthesia fails, from one reason or another, in approximately 8 per cent. of cases. (3) Complications are relatively common. A profound fall in blood pressure, with restlessness, anxiety, and vomiting, is a not infrequent accompaniment of the operation, while subsequent headache may be severe and prolonged. The pulmonary complications for which spinal anaesthesia was to be the panacea are at least as common as after inhalation narcosis: post-operative pneumonia complicated the cases reported by McKettuck[9] and Arnheim[10] in the proportions of 10 and 8 per cent. respectively, while Brown and Debenham[11] record that it is four times as common as with general anaesthesia. In addition, pregnant women, as Heim[12] has pointed out, are particularly liable to suffer from the complications above mentioned.

Intravenous Anaesthesia.—The popularity of evipan sodium injected intravenously is now almost universal, in spite of its very recent introduction. But its use is limited to operations of very short duration, for repeated doses of this drug entail many of the risks which accompany inhalation anaesthesia.

Rectal Anaesthesia.—For complete anaesthesia the rectal administration of avertin or paraldehyde is generally acknowledged to be dangerous. As a basal anaesthetic, with nitrous oxide as an adjunct, the method is an excellent one, although idiosyncrasies are not uncommon, and many anaesthetists dislike the finality of its retention. In addition, pulmonary complications are not infrequent.

Sacral Anaesthesia.—This method, originally described by Stoeckel,[13] is identical with " extradural," " epidural," and " caudal " anaesthesia. It has been successfully employed by Laewen,[14] Scholl,[15] Schlimpert,[16] Perrill,[17] and many others, including Schlimpert and Schneider[18] and Meeker and Bonar,[8] in gynaecological conditions. But, as the last-named writers admit, technical difficulties make the anaesthesia both unreliable and variable, and failures have been recorded by Fischer,[19] Pickens,[20] Kehrer,[21] and Babcock[4] in the proportions of 15, 17, 21, and 45 per cent. respectively. In addition, toxic symptoms are common (Hirschell[22]).

Parasacral Nerve-block.—This consists in novocain injection of the sacral nerves at their emergence from the bone. The needles are passed through the perineum along the front of the sacrum, and the nerves of each side injected at their anterior foraminal exit. Braun,[23] who introduced it, Toelken,[24] and Farr[25] have used this method with a considerable degree of success, but all agree that the technique is difficult and the results uncertain. There is also the risk of perforating the rectum, a particular danger in a region where sepsis is so serious a complication.

Trans-sacral Nerve-block.—With this form of anaesthesia the sacral nerves are infiltrated through the posterior sacral foramina. Pauchet,[26] Oliviera,[27] and Meeker and Frazer[28] employ it with success in gynaecological cases, but the technique is tedious and difficult ; and as it does not appear superior to low intradural anaesthesia, it is unlikely that it will be widely adopted.

Hypogastric Anaesthesia.—Similar objections apply to the posterior route hypogastric plexus injection of Frigyesi.[29] As Fekete[30] says, the technique is difficult and uncertain, and pulmonary complications are not uncommon. Also, the anaesthesia obtained is necessarily very limited, and has usually to be supplemented either locally or generally.

Advantages of Local Anaesthesia

Local anaesthesia is free from nearly all the disadvantages discussed above. There is practically no mortality. Rollison[31] found six deaths in 6,062 general and none in 1,981 local anaesthesias, and of 107 anaesthetic deaths collected by Kaye[32] only eight occurred under local anaesthesia. Both Frey[33] and Greenhill[34] have used this method in very large series of gynaecological operations, in both instances without fatality.

In addition, the more positive advantages peculiar to local anaesthesia should be taken into consideration in assessing its comparative value. First, haemorrhage is much diminished by the adrenaline content of the anaesthetic solution, this effect being of considerable advantage in the colporrhaphies, where oozing resulting from the mucous separation is uncontrollable by any other method. Secondly, the fluid intake by mouth can be continued before, during, and immediately after operation, thus preventing dangerous dehydration. Lastly, the patient gains, and local healing is accelerated, by the extra gentleness required of the operator by the avoidance of the painful effects of undue traction.

Technique

Premedication.—It is essential that the patient's perceptions be rendered insensible to the psychical and physical discomforts of the operation table. Scopolamine-morphine narcosis is often employed for this purpose, but I have found nembutal more satisfactory. It occasionally causes some restlessness, but it is free from the depressing and nauseating effects of morphine: 1½ grains are given the night preceding, and 3 grains half an hour before, operation. Particular care is taken to make the patient comfortable and, if conscious, to distract her attention from the operative procedures.

External Tumours and Cysts.—These may be removed by circular field block around and beneath the tumour, after the method of Braun. More extensive procedures on the vulva require complete circular field block of the part. This is performed by travelling the needles subcutaneously around the vulva from a starting-point at the central point of the perineum. Binet,[35] Ilmer,[36] King,[37] and Lilienfeld[38] state that block of the internal pudic nerve alone is sufficient to allow of the painless performance of superficial vulval and perineal operations. My own experience, however, is similar to that of Gellhorn,[39] who found the anaesthesia from this method both unsatisfactory and unreliable, an opinion also shared by the authorities Pauchet and Labat.[40]

Operations on the Cervix.—For dilatation of the os cervicis the injection is made directly into the pelvic plexus of Lee-Frankenhauser. I have elsewhere[41] described the anatomy of this plexus in detail, and it is only necessary to state here that it is a bilateral, flat, fenestrated, quadrilateral mass of nerve fibres lying on either side of the anterior portion of the ampulla of the rectum. The cervix is seized with vulsellae, steadied by very gentle slight traction, and held in this position by an assistant. The needle, which is a very long one, is passed into the vagina along the forefinger of the right hand, until it impinges on the postero-lateral fornix. The vaginal wall is then perforated at this point by gentle, firm pressure, and the needle, guided by a finger in the rectum, is passed upwards and backwards for 1 cm.: 10 c.cm. of a 1 per cent. solution of novocain are then injected, the needle-

point being kept continually moving and perforation of the rectum being carefully avoided. The procedure is then repeated on the other side.

By this method cervical dilatation is quite painless. Its efficacy is well demonstrated in those cases in which, for some reason or other, it is impossible to inject both plexuses, when the dilatation is accompanied by severe pain on the unanaesthetized side only. It is particularly useful for the insertion of radium according to the Heyman technique, avoiding as it does the necessity for three separate anaesthetics. The method is also of value for cautery of the cervix and insufflation or lipiodol injection of the tubes. Perforation of the uterine artery is, of course, possible in this injection, but it is rare and apparently of little importance. I have only once encountered this complication, which resulted in that case in a small, symptomless, parametric haematoma. If the needle is moved continuously during the injection there is very little danger of collapse from the entrance of novocain into artery or vein, as in the two cases encountered by Braun. For curettage the above technique should be supplemented by the direct application of novocain (on a gauze strip) to the uterine endometrium, which sometimes remains sensitive.

In trachelorrhaphy and other cervical operations, although pelvic plexus infiltration alone is often sufficient, it is desirable as a routine to inject additionally, (*a*) the whole lateral parametrium, and (*b*) the cervix itself. The former is performed at the same time as the plexus injection, and eliminates the pain due to a certain amount of inevitable cervical traction. The latter is a submucous injection all round the base of the cervix, and is useful in its haemostatic effect. It has, in addition, a definite anaesthetic value, for by this means afferent impulses are interrupted from the sensory structure I have elsewhere described as the "subepithelial plexus of the tip."

Colporrhaphy.—In anterior colporrhaphy the pelvic plexus and parametric injections are combined with a generous submucous infiltration of the whole anterior vaginal wall. A further injection is made beneath the bladder before its separation. It is useful, in addition, to anaesthetize the terminal urethra and the meatus with gauze strips soaked in 2 per cent. cocaine. A small posterior colporrhaphy may be carried out with a similar anaesthetic technique to the above, but for more extensive procedures it must be combined with block infiltration of the perineum. This method, which has been carefully elaborated by Braun, is also useful for uncombined *perineorrhaphy*, and is carried out as follows.

From a midline cutaneous wheal midway between vulva and anus, the tissue between the vagina and rectum is infiltrated with a long needle almost to the fornices. The needle is controlled by a finger in the rectum, and by its partial withdrawal and reintroduction in several horizontal planes a considerable area is anaesthetized. By directing the needle more and more transversely, a wall of infiltration is eventually built up across the upper part of the ischio-rectal fossa, thus effectively blocking the branches of the pudic nerve as they cross the fossa. Combined with parametric and pelvic plexus infiltration, and with the circular subcutaneous vulval block described above, this method gives a very satisfactory anaesthesia. Further relaxation of the pelvic floor, with consequent greater ease in approximating the levatores, is obtained by the injection of a few cubic centimetres of novocain directly into those muscles.

Pudic Nerve Block.—As stated above, direct block of the pudic nerve is itself of little value, but it is a useful adjunct to the technique described for perineorrhaphy, helping considerably to produce relaxation of the muscular floor of the pelvis. The nerve is reached as it lies along the medial surface of the inferior ischial ramus. The tuber ischii is identified, the needle plunged through the skin and subcutaneous fat in direct contact with the ramus, and 10 c.cm. of the anaesthetic solution injected against it. A further 10 c.cm. is then injected into the adjacent part of the ischio-rectal fossa. Franke and Posner[42] inject the pudic nerve as it lies on the spine of the ischium, but their technique is more difficult and uncertain than the one just described. It has the advantage of avoiding the possibility of pressure-necrosis of the badly nourished fat in the ischio-rectal fossa, but this appears to be a purely theoretical risk.

Conclusions

1. While in no way advocating the widespread substitution of local for general anaesthesia in gynaecology, it is suggested that a considerable number of minor operations may be advantageously performed by this method. It is probable that the majority of patients are psychologically unfitted for local anaesthesia, but there remains a number whom it would benefit considerably.

2. It is suggested that the increased safety and efficacy of the various sacral anaesthesias do not compensate for the difficulties and uncertainties of these methods.

3. It will be noticed that no mention has been made above of the performance of vaginal hysterectomy or of any abdomino-pelvic operation under local anaesthesia. I believe that the latter is unsuitable in every way for both these types of procedure, which are more adequately carried out under low spinal anaesthesia.

4. Local anaesthesia is easy, safe, and efficacious. Its use is necessarily restricted, but it has many advantages over other methods in selected cases.

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FATAL CASE OF LEPTOSPIRAL JAUNDICE OF OBSCURE ORIGIN

BY

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AND

M. J. STEWART

LEEDS

It has seemed to us that the following brief account of a recent fatal case of leptospiral jaundice occurring in Leeds might be of interest for several reasons. First, the recorded cases in this country have, with rare exceptions, been shown to be related to certain occupations in which frequent contact of the skin with water soiled with rat excreta was undoubted and difficult to prevent. Secondly, the laboratory diagnosis of the case was complete, as the guinea-pig was successfully inoculated with the patient's blood, and this method of investigation, although recommended as the ideal one for diagnosis of the disease, has rarely been used successfully in this country—for example, the series of cases in workers in the fish industry recently published by Davidson *et al.* (1934) and the earlier series of Buchanan (1927). The only two cases of the disease successfully diagnosed in this way in the British Isles of which we have found records are those of Manson-Bahr (1922) and Fairley (1934). Lastly, the aetiological problem presented was unusually difficult.

Case Record

The patient, a healthy, active, and athletic man of 27, was taken ill on Thursday, September 7th, 1933, with severe headache and a temperature of 103.6° F. He vomited once, and the following day he had a slight rigor. When first seen by one of us (G. W. W.) on Saturday, September 9th, he was complaining of slight pain in the throat, which extended to the ears on swallowing; pain in the occipital region and back of the neck (not severe, but increased on flexing the head); and some discomfort in the back. Examination of the throat showed nothing very definite, and a swab failed to reveal the presence of any organisms of importance. On examination the conjunctivae were injected, but otherwise neither the eyes nor the nervous system generally showed anything abnormal. It is to be noted that the abdominal reflexes were brisk at this time.

The following day (Sunday, the 10th) there was little change in the general condition, and certainly no improvement. The pain in the back was, if anything, rather worse. The breathing was of a sighing character. By Monday, the 11th, the complexion had become faintly icteric, and there was pain and tenderness in the legs. The pain in the back was worse and the abdominal reflexes had disappeared. That evening there was retention of urine, requiring the use of the catheter. Spinal puncture yielded clear fluid under a little pressure, but cytological and bacteriological investigations were negative.

On Tuesday, the 12th, there was definite jaundice with bile in the urine. The retention of urine persisted, and there was some abdominal distension. There was much pain in the legs, and even slight manipulation caused great distress. On the following days the jaundice became much deeper, and the abdominal distension increased. Regurgitant bilious vomiting commenced and became increasingly troublesome. Increasing apathy and drowsiness without muscular twitching or tremor marked the end. Only during the last thirty-six hours had there been any haemorrhage into the skin. The spleen and liver had not been palpated at any time.

Death occurred early on the morning of Monday, September 18th, eleven days from the onset of symptoms.

NECROPSY

The body was intensely jaundiced, but muscular and well nourished. The *liver* (64 oz.) was pale, yellowish, and soft, showing much less bile-staining than the kidneys and no evidence of necrosis. All the main *bile-ducts* were patent and contained mucus only. The *kidneys*, slightly enlarged, were notably bile-stained; their capsules stripped easily, leaving a smooth surface. The *stomach* contained a quantity of altered blood, the *oesophagus* adherent ante-mortem thrombus in its lowest three inches. There was an ancient calcified tuberculous gland in the mesentery. The *lungs* showed tiny patches of haemorrhage in their substance, and there were punctiform haemorrhages also in the *pleurae* and *pericardium*. The *heart muscle* was yellowish, soft, and flabby. The *appendix* had been removed three years before. None of the organs showed evidence of pathological change.

HISTOLOGY

1. The *liver* shows a moderate grade of subacute hepatitis in the form of cellular infiltration of the portal tracts, some being notably affected, especially the medium and smaller, others very slightly. The infiltrate is largely polymorphic, with varying numbers of lymphocytes and plasma cells, and a few eosinophil cells. There is no liquefaction of tissue, however, and the term "suppuration" cannot be applied to it. The hepatic parenchyma shows definite changes: (*a*) intracellular bile stasis in the central zones of the lobules; (*b*) a general loosening and detachment of the epithelial cells with slight early necrotic changes in a few only; and (*c*) mitotic figures in small numbers. It is to be noted that there is no biliary obstruction in even the smallest intercellular canaliculi—only within the cells themselves.

2. The *kidney* shows a very notable accumulation of bile pigment both in the convoluted tubular epithelium and in the lumina of the tubules throughout both cortex and medulla. There is some evidence of toxic change, otherwise the kidneys show nothing of note. Small foci of plasma cells and polymorphs are present here and there.

3. In the *spleen* the pulp is congested and very cellular. The Malpighian bodies show a definite focus of hyaline degeneration at the centre, which does not take the amyloid stain.

4. The *heart muscle* shows fragmentation of muscle cells, otherwise nothing of note.

5. The *lung* contains a number of small vessels filled with purulent plugs, while others show polymorphic infiltration of the perivascular lymphatics. There is much patchy haemorrhage and oedema. Some of the bronchi contain a trace of pus, but the condition is essentially an acute interstitial inflammation, probably spreading by the blood vessels or lymphatics, or both.

Summary.—The principal lesions present are therefore subacute hepatitis with intracellular bile stasis, and acute interstitial pneumonia.

BACTERIOLOGICAL FINDINGS

The case was first seen by the bacteriologist on the fifth day of the disease. On account of the existence of definite jaundice combined with a febrile condition and very marked prostration, it was thought desirable to inject some citrated blood from the patient into the peritoneum of the guinea-pig, as well as to do a blood culture. No attempt was made to cultivate the blood under conditions suitable for the growth of leptospira, and a negative result was recorded on cultivating in the medium used for routine work. When the patient died a few days later with pronounced icterus, preparations of liver juice were examined for leptospira under dark background, but without success.

Eleven days after its inoculation with the patient's blood a guinea-pig, one of two injected, developed icterus and died the following day, its fellow having died prematurely. Leptospira were not detected in preparations of its viscera observed under dark background, but two guinea-pigs injected from it died in eight days. Again we failed to detect spirochaetes in the liver juice of these animals. The third pair of guinea-pigs injected in series died in six days, and in their viscera typical leptospira were readily demonstrated under dark background, and were also recovered in culture on the usual semi-fluid Locke serum-agar media incubated at 30° C. for a week. For the successful cultivation of the leptospira we owe much to Mr. G. N. Boxhall, B.Sc.

The difficulty encountered in demonstrating the leptospira in the experimental animal at the outset may have been partly due to lack of experience in observing these

organisms in the living state, since sections prepared by Levaditi's method revealed their presence in the viscera of the first guinea-pig in the series. There is in any case a great contrast between the sharpness with which these micro-organisms are defined in culture and their relative elusiveness in the tissue juices, and it may be useful to laboratory workers unfamiliar with these forms at the outset of their investigations, as we were, to draw attention to it. When you have once seen them, the extremely characteristic movements of the leptospira are very helpful in their recognition. We found it notably difficult to get a convincing preparation by staining smears from tissues or cultures.

Discussion

The most interesting problem in connexion with this case was the source of infection, since it was entirely isolated, no case having been recorded in Leeds or its vicinity for many months. The most plausible suggestion, and the only one which seems to have any claim to attention, is that this was a bathing infection.

The patient was in the habit of bathing in the Yorkshire streams as and when he had the opportunity, and especially in the River Wharfe and some of its tributaries. In the second week of July, 1933, he went for a fishing holiday to Upper Wharfedale, and bathed frequently in one of the becks. He complained of being unduly tired at this time, and he had a long-standing abrasion on one heel. The last occasion on which he bathed—in the Wharfe at Ilkley—was on September 3rd, four days before he was taken ill. The river was unusually low at the time.

The French observers draw attention especially to the importance of contact with mud rather than water (Vincent, 1933), but Troisier, Bariéty, Erber, and Gabriel (1934) mention the possibility that swimmers habitually passing through dirty water may acquire immunity from latent or mild infections, and show abnormal agglutinating powers in their sera for suspensions of leptospira.

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According to L. Fischer (*Zentralbl. f. Gynäk.*, February 2nd, 1935, p. 249) in the pre-insulin era the comparatively rare cases of gestation in diabetic women almost invariably gave issue to over-weight children, of whom two in five were "giant children"—that is, weighed over 5 kilograms. In a series of thirty-one recent collected cases of childbirth in diabetics who had been treated with insulin there was, however, no "giant child": that some of the children were over-weight is explained by the fact that not all the mothers had received sufficient insulin treatment. In pre-insulin days an improvement of the maternal sugar metabolism was not infrequently noted towards the end of pregnancy, and this has been attributed to the activity of the foetal pancreas in making up the deficient insulin production of that of the mother. Anatomical and biological evidence has been adduced, from many sides, of the insular activity of the foetal pancreas in the second half of pregnancy. A more probable explanation, according to Fischer, is that the foetus abstracts from the maternal circulation considerable amounts of sugar, which are converted in the foetal circulation to glycogen and indirectly to deposited fat. Probably the insulin molecule is too large to pass through the placenta, and certainly "giant children" show insular hyperplasia and hypoglycaemia.

RADIOLOGY OF THE APPENDIX

(APPENDICULOGRAPHY)

BY

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There are many cases of obscure abdominal pain in which an accurate *x*-ray representation of the appendix may be of considerable aid in the diagnosis. It is not generally realized that this can now be obtained in all cases in which the lumen of the appendix is not obliterated by disease or obstructed. The method of filling the organ is relatively simple, and consists in giving a preparation of barium with milk or cornflour at night, following this after an interval of three hours by a dose of magnesium sulphate varied according to the requirements of the individual patient.[1] Radiograms are obtained on the following morning, and are continued at such intervals as may appear necessary during the following days, until the caecum is entirely empty.

What are the normal appearances of the appendix in a radiogram? Rönsberg[2] states that the appendix may be considered as normal if it lies in its usual position, is freely movable, has a smooth outline, fills rapidly, and empties at the same rate as the large bowel. Not every normal appendix fulfils these requirements.

Position.—There is no definite position for the appendix, but it is most frequently identified medial to the caecum. It may point upwards, downwards, or transversely, and may even lie lateral to the caecum; in the latter case it is probably abnormal.

Contour.—When completely filled, the walls are smooth in outline until emptying commences. The presence of peristalsis then gives it a segmented appearance rather like a string of small sausages.[3] Incomplete filling can impart an irregular appearance to a perfectly normal appendix.

Emptying Rate.—The appendix normally empties at the same rate as the caecum. Any delay in emptying after forty-eight hours is suggestive of an appendicular lesion.

Mobility.—If the appendix is identified on screen examination it can be moved by palpating with the hand, and it will be seen that it moves freely in conjunction with the caecum. However, if it is of sufficient length it will also have an independent mobility of its own. In the serial radiograms it will be seen changing its position under the influences of peristalsis rather like a pendulum. This, however, never occurs if the meso-appendix is short.

Filling Defects.—The barium-filled normal appendix frequently shows partial filling defects apparently due to the presence of faecal material. Filling defects may, however, be due to the presence of faecaliths, when they will be more sharply defined and constant in position.

In a series of one hundred patients examined during the last two years, the appendix was successfully filled in ninety-three and failed to fill in seven. Thirty of the patients were operated upon, and the *x*-ray appearances correlated with the operation findings and the pathological reports. In twenty-eight cases gross and obvious changes in the appendix were noted at operation, whilst the remaining two showed no evidence of abnormality.

Case Records

The following seven cases are selected from the series in order to demonstrate the changes seen in the radiograms in disease.

CASE I

Male, aged 29, complained of abdominal pain and loss of weight with attacks of indigestion during the last eleven years. *Physical examination :* abdomen soft, no rigidity, no tenderness. *Barium meal examination :* the stomach showed no radiological evidence of abnormality. The radiograms, which were taken at six and eleven hours after the ingestion of the meal, show the appendix filled (see Fig. 1). It formed an S bend with a rather sharp angle near the base. The distal end was incompletely filled. The appendix retained this shape throughout the examination, and was also seen filled after the administration of a barium enema, when a similar shape was observed suggesting that it was fixed in position.

Operation.—The appendix was found to be bound with adhesions from end to end.

FIG. 1.

FIG. 2.

FIG. 3.

FIG. 1.—Eleven hours after the barium meal.
FIG. 2.—Twenty-four hours after the barium meal.
FIG. 3.—Twenty hours after the barium meal.

CASE II

Female, aged 19, complained of pain in the right lower abdomen which she had had for some months, and a feeling of sickness accompanying the pain. *Physical examination :* no localized areas of tenderness. *Barium meal examination :* the stomach examination proved to be negative, and an *x*-ray examination of the appendix was suggested owing to the site of the pain. At twelve hours after the meal the appendix was not visible, but at twenty-four hours it was filled, and could be seen lying close up against, and parallel to, the base of the caecum, with its tip turned upwards (see Fig. 2). Radiograms taken at thirty-four and thirty-eight hours showed the appendix still filled after the caecum was practically empty. There was no change in the position of the organ suggesting fixation by adhesions.

Operation (Fourteen Days Later).—The appendix was found bound down by adhesions. The symptoms disappeared after the operation.

CASE III

Female, aged 12 years, was admitted to a sanatorium under suspicion of tuberculous peritonitis. *Barium meal examination :* radiograms were taken of the appendix owing to localized pain in the right iliac fossa. The appendix was seen filled at twenty hours (see Fig. 3), and it appeared abnormally large, with a definite dilatation of its extremity. There was retention in the organ up to four days, at which time the caecum and colon were quite empty. The abnormal retention and dilatation of the appendix suggested the presence of a lesion of the appendix.

Operation.—Three months after this examination the condition became acute and appendicectomy was performed. The appendix was found to be acutely inflamed.

CASE IV

Male, aged 25 years, gave a history of attacks of pain in the right lower quadrant of the abdomen. *Barium meal examination :* stomach negative. The appendix was seen filled with barium at twenty and thirty-six hours after the ingestion of the meal, and obvious filling defects were visible in it (see Fig. 4). There was abnormal retention of barium, but the motility of the organ appeared to be unaffected. The *x*-ray appearance suggested a lesion of the appendix.

Operation and Pathological Report.—The appendix was found to be long and dilated, but not fixed. It was filled from end to end with faecaliths. No outward sign of inflammation could be seen. The section showed evidence of hyperaemia of the lymph nodes, indicating the presence of inflammation.

CASE V

Male, aged 45 years, complained of attacks of acute pain in the lower abdomen during the last five months, independent of meals but made worse by purges. Improved with bismuth powder. *Physical examination :* there was tenderness across the lower abdomen, more marked in the right iliac fossa below McBurney's point. *Barium meal examination :* radiograms were taken at twelve (see Fig. 5), twenty-two, and thirty-six

FIG. 4.

FIG. 5.

FIG. 6.

FIG. 4.—Thirty-six hours after the barium meal.
FIG. 5.—Twelve hours after the barium meal.
FIG. 6.—Seventeen hours after the barium meal.

FIG. 7.

FIG. 7.—Twelve hours after the barium meal.

hours after the ingestion of the meal, and the appendix was visible at each examination. It was dilated at the tip, fixed in position, and had a residue at thirty-six hours, when the caecum was empty. The appearances were suggestive of an appendix lesion.

Operation.—The appendix was found exactly in the position indicated in the *x*-ray films. It was adherent throughout its length and dilated in its distal third, which contained faeces. Adhesions were present between the ileum and the lateral abdominal wall. There were definite signs of chronic appendicitis.

CASE VI

Male, aged 31 years, complained of a burning sensation in the epigastrium after food during the last six years, with some pain in the right iliac fossa during the last two years. Referred to *x*-ray department for investigation —" ? gastric ulcer ; ? chronic appendix." *Barium meal examination :* the stomach was dilated and atonic. There was no evidence of ulceration. The appendix was seen filled at seventeen (see Fig. 6), twenty-six, forty-one, and fifty hours, and retained the barium after the caecum was empty. The organ was fixed and dilated at its extremity.

Operation and Pathological Report.—Adhesions were present between the appendix and the caecum. There was naked-eye evidence of subacute inflammation. The appendix contained a considerable amount of debris in its lumen, with red blood cells. There was evidence of round-celled inflammation, and the appearances suggested the presence of subacute inflammation.

CASE VII

Male, aged 64 years, complained of colicky pains in the right lower abdomen. *Barium meal examination :* the appendix was seen filled at twelve (see Fig. 7), twenty-two, thirty-six, and forty-eight hours. The lumen was dilated, and filling defects were present. The organ appeared abnormally long and relatively immobile. The appearances were suggestive of a lesion of the appendix.

Operation.—The appendix was filled from end to end with faecaliths. There was no sign of inflammation or adhesions.

Discussion

The radiologist is not always asked to examine patients suspected of appendicitis. This is in part due to the fact that the radiology of the appendix has been up to now suspect. One of the reasons for this is that in the past we have failed to fill the normal appendix owing to defective technique, and there has been a tendency to rely on indirect signs such as pyloric spasm, abnormal filling of the caecum, and localized tenderness, etc. These indirect signs are frequently misleading. An unfilled appendix should only be diagnosed as abnormal if it fails to fill after repeated examination.

The normal appendix gives characteristic appearances in the radiogram, and, *per contra*, the abnormal appendix can also readily be detected, as a rule, from alteration in its contour, emptying rate, position, and other changes. Adhesions are revealed by fixation of the appendix in an abnormal position in relation to the caecum, or by the retention of unusual curves or kinks in the serial radiograms. It must be remembered, however, that in many patients without signs of disease there is diminution of the swaying or pendulum movements of the appendix. The presence of adhesions must therefore be diagnosed with caution, the more so since a short meso-appendix will limit the movements of the organ.

A large single faecalith, if it contains calcium, may be visible in a plain radiogram, and is usually seen close to the right iliac crest.[4] But it is more often the case that the faecaliths when present are invisible, and are only detected as filling defects in the barium-filled appendix. Inconstant filling defects are often seen in normal cases, and are apparently due to accumulated secretion. The filling defects caused by faecaliths retain their contour and position in all the radiograms. The lumen is generally slightly enlarged where many faecaliths are present.

The most characteristic changes are seen when the appendix is the seat of subacute inflammation. There is obvious dilatation of the lumen, especially at the distal extremity, as in Case III; fixation is present in one position, and, owing to the diminution or absence of peristalsis, there is delayed emptying of the barium, the appendix being generally the last part of the bowel to empty. Localized tenderness is frequently present.

When the appendix lies behind the caecum it is concealed from view in the earlier radiograms. As the caecum begins to empty, however, it comes into view, and it is then sometimes possible to obtain films showing its shape and position. These cases are some of the most difficult from the point of view of the radiologist. The size of the lumen of the appendix appears to vary with the condition of the colon. When the colon is spastic the appendix tends to be contracted and the lumen is narrow. On the other hand, when the colon is found to be atonic, the atony appears to affect the appendix at the same time, and it fills with considerable ease and shows a large lumen. It will often fill in such a case under the pressure exerted by a barium enema. This, however, is not usual, since the appendix is normally only visible after the enema has been partially evacuated. Dilatation of the lumen of the appendix appears to be one of the most important radiological signs of disease, especially when it affects the distal extremity and is accompanied by retention of the barium.

Summary and Conclusions

1. It is suggested that the appendix, unless blocked by fibrosis or adhesions, will always be filled with barium if careful attention be paid to the radiological technique.

2. A careful study of the radiographic appearances of the filled appendix enables a reasonably accurate radiological diagnosis to be made in most cases.

3. The following radiological signs are suggestive of disease: (*a*) dilatation of the lumen, (*b*) fixation in one position, (*c*) delay in emptying, (*d*) clear-cut filling defects, and (*e*) non-filling after repeated examinations.

I should like to take this opportunity of acknowledging the assistance received from Mr. Rast and other colleagues at the German Hospital who have provided me with clinical material and operation results.

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SOME OBSERVATIONS ON SURGICAL URAEMIA

BY

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The surgical type of uraemia may be defined as that state which supervenes when there is disease or functional derangement of the urinary tract and a failure to excrete toxins present in the blood and tissues of the patient. This implies, first, a derangement of the urinary mechanism, and secondly, an accumulation of toxins as the result. I shall therefore limit myself to a type of case in which there is obstruction to the urinary outflow, accumulation of toxins, and no known previous disease of the kidneys themselves.

In the cases seen in the surgical wards the condition as defined above arises from two causes: (1) mechanical obstruction, most frequently seen in the peripheral passages, as in prostatic cases, and (2) septic changes which involve the kidney itself and injure its function. The latter group of phenomena are often superimposed on the former and complicate the picture, so that for the purposes of this discussion I have concentrated on the former type in which uraemia, uncomplicated by sepsis, is seen in obstructed cases.

Pathology of Urinary Tract Obstruction

What are the toxins which accumulate in a case of urinary obstruction? From time to time someone claims to have isolated a body which is asserted to be the substance responsible for the uraemic state; so far, however, no such single specific substance has been discovered which stands the test of critical examination, and it is probably safe to assume that the phenomena are due to a general accumulation of products, which are normally rejected, but which clog and derange the working of the whole organism by their presence in excess. Among the products accumulating in this way, the ones which primarily focus our attention are due to protein metabolism, and are all of a nitrogenous character. We may regard the condition as due to the soaking of the various tissues of the body in waste metabolic and katabolic products, so that we ought to go a stage further than the examination of the blood, and determine, if possible, when uraemia develops in relation to the accumulation of waste products in the tissues. This is a difficult subject, and there are as yet no definite data available.

The following facts emerge: in the first place, cases with high blood nitrogen and no symptoms of uraemia are common; secondly, a persistent high blood nitrogen ultimately leads to uraemic manifestations; and thirdly, after the removal of the obstructing cause the blood nitrogen does not always show an immediate fall, but a

gradual one. There is therefore a latent period between the rise of nitrogenous products in the blood and the onset of uraemia, and also a period of high blood nitrogen after the removal of the causative obstruction, whilst the tissues are unloading their accumulations and the excretory organs are unable to carry these away fast enough.

With regard to the determination and recording of these cases I shall merely mention the methods of estimating renal function. The important factors are the general clinical condition of the patient, the concentrating power of the kidneys, the daily fluid output, and the level of urea in the blood. The last-named must be closely watched in the presence of known defect of function.

Post-mortem microscopical examinations of these cases reveal no appearances suggestive of a specific derangement of the uriniferous elements. The glomeruli and tubules retain their characteristics, and are only influenced by the general congestion or the presence of an inflammatory infiltration throughout the organ. They may be the subject of atrophic changes due to the presence of fibrotic elements after sepsis or to the pressure of a hydronephrotic sac. Even in the septic cases the changes are clearly those which are due to the presence of the septic process, and are not limited in any way to the essential portions of the urinary mechanism. There is nothing, therefore, in these cases, so far as the kidneys are concerned, to account for the state of uraemia; and when we come to contrast the clinical picture of the surgical with the medical type of uraemia we observe that we are dealing with a somewhat simpler picture.

Clinical Picture of Uraemia

The kidneys having been deranged there is a general cutting down of the excretion of the substances presented to them for elimination. These accumulate in the blood, and are insidiously soaked up by the tissues until a hypothetical point of tissue saturation is reached, when the symptoms of uraemia supervene. Such a state is seen in the many recorded instances of complete calculous anuria, the striking feature of which is their relative symptomlessness. The patient remains in a comparatively quiet state, is mentally alert, is nauseated, vomits from time to time, tends to be rather drowsy, and has a dull headache. There is little else for a week or ten days, and then the rapid onset of coma and death.

In the more chronic cases a typical uraemic patient, as we see him, is frequently a man of prostatic age with some urinary obstruction. He has been carrying on his usual life until increased frequency of micturition, dysuria, or an attack of retention drives him to seek relief. His unstable condition is perhaps immediately revealed, because the mere upset of admission to hospital or the use of a catheter may throw him into a state of mild mental confusion. It will be discovered that for a time he has been losing weight and strength, and has been growing restless in his habits. He has been irritable, off his food, and his mental alertness may have been noticeably impaired.

On examination he is seen to have a yellowish, unhealthy appearance; the pulse is elevated, 80 to 90; the tongue is dry and dirty; and the daily output of urine 20 to 30 ounces. The urea concentration is below 1 per cent., and the blood urea 90 to 100 mg. per cent. or more. Such is the common picture. Occurrences such as wild delirium, convulsions, deep coma, etc., are absent. Occasionally one meets with abnormal symptoms, such as in a case which occurs to me, in which an acute abdomen was simulated. There was, however, an advanced degree of sepsis in this case, and one suspects that the unusual happenings are often due to simultaneous lesions, among which sepsis is of the greatest importance.

The genito-urinary surgeon is familiar with many cases of uraemia which present some symptoms of the medical type, but they are in a minority. To take an example, I would suggest that elevation of blood pressure is evidence not of uraemia, but of the coexistence of a true nephritis in addition to the state arising from the urinary obstruction. There is no rise of blood pressure in cases of total nephrectomy in animals or in human beings.

It appears, therefore, that uraemic cases of the surgical type present a less complex picture than do those seen in cases of true intrinsic disease of the kidneys. There are fewer symptoms, and these are usually milder. We appear to be dealing with a complete or incomplete cutting off of the ability to excrete the poisonous metabolic substances in the blood. The "excretory tap" is turned off, as it were, absolutely or partially, permanently or temporarily.

Effect of Complete Anuria

Let us see what happens when the excretory flow is shut off absolutely. Double nephrectomy in animals has shown that the symptoms are not those of the active uraemic type as occurs in cases of intrinsic nephritis. There is no production of oedema, the metabolic products accumulate in the blood, and the animal dies frequently without any pronounced symptoms.

In this connexion the case has been described by Thompson in the *British Medical Journal*[1] of a young man who injured his kidney playing football. Haemorrhage could not be controlled, and a nephrectomy was performed. Complete anuria supervened, and it was found that he was a case of solitary kidney, which had been excised. He lived for ten days, during which he had no convulsions. His symptoms were a dull headache, a good deal of vomiting, fibrillary twitchings of the muscles, drowsiness, and a terminal coma during the last one or two days. During this period the blood showed the following changes:

	At commencement	Terminal
Non-protein nitrogen in mg. per 100 c.cm. whole blood	20	168
Urea " " "	20	272
Ammonia " " "	0.1	133
Chloride " " "	450	351
Ca : P ratio " " "	4	0.53
Alkali reserve in c.cm. CO_2 bound by 100 c.cm. plasma	50–90	39
Blood pressure in mm. Hg	116/62	110/40 (day before death)

I will describe a parallel case in which the kidneys were destroyed by lithiasis until terminal uraemia resulted in death.

A young woman had had stones removed on three or four previous occasions from both kidneys, and when I saw her for the first time on the last day of her last illness both pelves and both ureters were again blocked almost solidly with calcareous material. The urinary output had fallen to a few ounces of low specific gravity fluid, which was little else but water. She was nauseated and vomiting from time to time. She was particularly clear mentally, and only distressed by the vomiting. Blood urea was well over 200 mg. per cent., and she died a few hours after I saw her and advised against operation.

These two cases are alike in that in both renal function was completely cut off: in the first by the removal of the remaining kidney, and in the second by stone. Both exhibited similar terminal symptoms.

The Everyday Case

Such are extreme cases, but the same appears to happen in our prostatic uraemic cases. There appears to be a "turning off of the excretory tap," irreversible in the two cases I have just mentioned, but as I shall now show reversible in many cases of lesser degree. The

[1] December 24th, 1932, p. 1134.

majority of mildly uraemic cases, such as one sees in prostatics, obstructions due to stricture, calculi, etc., only fail to make a good recovery in a minority of instances. The prognosis in these cases depends on the amount of real destruction of renal substance which has occurred, and not so much on the actual presence of uraemic symptoms.

I will give a short description of three cases which illustrate some of these points.

Case 1, a taxi-driver aged 54, was admitted under my care in 1923 with acute retention due to an impassable urethral stricture. There had been complete retention for thirty-six hours, and on admission his general condition was extremely bad. He was unable to answer questions, his tongue was dry and furred, and he was nearly comatose. As no catheter could be passed a suprapubic cystotomy was done, no attempt being made to deal with the stricture. After two or three weeks' severe illness he recovered completely, had his stricture excised, and is now as well as ever he was. He went back to his work as a taximan, and still comes to the hospital once or twice a year "to make sure that his stricture is not re-contracting."

Case 2 was that of a man of 62 with retention due to enlargement of the prostate. His general condition was poor, with a urea concentration below 2 per cent., and as he did not respond to catheter drainage he was opened suprapubically. For a short time he did well, but for financial reasons was anxious to leave the nursing home, and so passed out of my direct observation. He must have got into a condition of mild uraemia, because such reports as I had of him were that he would not eat, was irritable, and generally miserable. He gradually became worse, and after about two months was persuaded to come into hospital. It was then noticed that the daily output of urine was below 20 ounces, and the blood urea was found to be above 200. The hospital residents thought that he would die. The whole difficulty was that, although the suprapubic was working well, he was refusing to take sufficient fluid by the mouth. His error was pointed out to him, and after some persuasion an adequate diuresis was established, and he made a perfect recovery.

Case 3, occurring nine months ago, was that of a man with prostatic obstruction and renal insufficiency. A preliminary suprapubic was done, but the expected recovery did not take place, and he developed a persistent high pyrexia. Further investigation revealed a small bilateral hydronephrosis, which was infected. The pyrexia continued for nearly four months, in spite of a variety of treatments, and it was at last decided to drain one of the kidneys, as it was thought that the drainage by way of the ureters might not be sufficient. A right nephrostomy was done, from which urine appeared twenty-four hours later. The pyrexia disappeared, and after a few weeks' drainage I was able to deal with the prostate by endoscopic resection. Now both suprapubic and renal sinuses have closed, he feels well, and is back at light work.

Lastly, Professor Jean Brau of Montpellier, at a medical congress at Vittel in 1930, quoted a case of anuria which came on after the removal of a pelvic tumour. Three days later a nephrostomy was done on the left side, which acted successfully and saved the patient. The right kidney, however, did not recommence its activities for thirty-four days.

Object of Treatment

A perusal of these cases—which one could multiply indefinitely—three of which are from my own practice and one from the literature, demonstrates several interesting facts. We have seen that in the obstructed cases there is a "turning off of the excretory tap" by the kidneys, more or less complete according to the type of case. In mild established prostatic obstruction the decrease of renal activity is moderate, whilst in the anuric cases it is practically complete, and the picture compares with those of absolute destruction or ablation of the kidneys.

We have seen that on adequate measures being taken the renal apparatus returns to normal, and in many instances often appears to be as good as ever. One is led, in fact, to infer a functional retardation of excretion rather than actual damage as the first factor when the kidneys are placed in difficulties due to obstruction lower down in the urinary passages, the bulk of the renal substance remaining potentially normal. Actual destruction supervenes as time passes in accordance with the particular circumstances of the case.

To turn on the "excretory tap" we must first secure adequate drainage. In many cases an indwelling catheter for some days will suffice; if not, a suprapubic fistula must be provided. It is essential to have a perfectly free outflow, and often a catheter is not enough. In addition, the intake of fluid must be forced. I try to get my patients to drink two to three quarts of water, preferably of the diuretic type, during the twenty-four hours. I keep them on a light diet and give them alkalis, which I think helps to establish diuresis. Of these two essentials, the necessity of forcing the fluid intake was noted in the case of the man with the suprapubic (Case 2, above), who did not take sufficient fluid, so that his blood urea rose above 200 mg. and fell only when enough liquid was given. With regard to the other essential, if, for any reason, suprapubic drainage is insufficient, as it may be in hydronephrotic or infected cases, a nephrostomy may have to be done before the kidneys will respond, as I have shown in Case 3.

Discussion

So far, we have seen that the surgical type of uraemia appears to result from a functional retardation, and that it is comparatively easy to reverse this unless there is severe destruction of the renal substance. It will be useful, therefore, to consider briefly the possible causes which govern this inhibition of renal function with a view to possible improvements in treatment.

It is doubtful if the time-honoured theory of "back pressure" really gives us an adequate explanation, because in many cases the uretero-vesical valves are efficient, actual dilatation of the upper urinary passages is absent, and yet uraemia may occur without any evidence of urinary reflux. Back pressure probably only contributes its share in the later stages of gross alteration of the urinary mechanism. Again, the accumulation of toxins due to the stagnation and fermentation of the urine in the bladder, while probably playing a part in contributing to the general renal discomfort, cannot be regarded as a complete explanation. There is little evidence of the early absorption of toxins from the bladder; in many cases the renal pelves and ureters appear to remain intact, and yet uraemia occurs. Further, the highly specialized renal cells can remain in their functionless state for much longer periods than one would suppose probable if they were rendered inert by poisoning, and yet they recover immediately on the removal of the causative influence. One cannot put the effect of toxins on one side, but I do not think it gives us a complete explanation of the whole problem.

A third suggestion has not yet been fully explored, and I feel that until it has been much further investigated we shall not reach a complete solution of our difficulties. The phenomena of renal excretion are much more dominated by the sympathetic nervous system than has yet been realized. We have seen that the accumulation of the metabolic products in the blood and tissues is due to a sort of arbitrary shutting down of the renal activities, and that the microscopical examination of the kidneys reveals no sufficient reason for this behaviour. We know also that this suspension of activities can be brought about, without any apparent

disease of the organs, by a simple stimulation producing a partial or complete anuria, as in the well-known instance of a calculus in one ureter causing anuria not only of the affected side but also of the opposite one; uraemic symptoms may occur when no appearance suggestive of obstruction can be found; such a simple proceeding as the passage of a catheter often appears to be the immediate cause of a tendency to anuria in predisposed cases; and a period of anuria is sometimes provoked by manipulations of the ureters by the passage of ureteric instruments. One may recall here Case 3, in which the manipulations of the kidney, necessary to establish a nephrostomy, were sufficient to cause complete anuria for twenty-four hours, after which it excreted freely.

One is inclined to suggest, therefore, that stimuli from a prostate, stricture, calculus, etc., may provoke a primary reflex inhibition of renal function by way of the sympathetic nervous system, the degree of inhibition being in proportion to the severity of the causative stimulus. It is difficult to believe that cells such as those which make up the renal excretory apparatus can be rendered functionless for prolonged periods by toxic influences alone without suffering permanent damage, and yet recovery is often rapid and apparently complete. Poisoning of the renal cells must often, of course, exist, and to it one must attribute the permanent damage which occurs, but I feel certain that for a full understanding of these cases evidence of the effect of irritation, mechanical or toxic, on the sympathetic control of the kidneys must be sought for before we shall be able to give an exact explanation of all the phenomena. It is noteworthy that the inhibitory effect of toxins on the nervous tissues of the brain is often prolonged, and recovery is frequently slow. It may be months before symptoms such as those of mental depression disappear and the patient is normal.

In illustration of this, and in conclusion, I will quote a portion of the preface of a novel, *Aphrodite in Aulis*, by George Moore, and dedicated to Sir John Thomson-Walker, who had removed the author's prostate:

"At the end of two months and a half I staggered out of the Home into a cab, a shrunken piece of wreckage, craving for the seashore. The sea and its salt breezes restored me to health, and soon I began to write; but I burnt as much as I wrote, and when you came to see me before leaving London for a holiday, I told you that I had given up all hope of writing *Aphrodite in Aulis*. 'The words won't stick together,' I cried, with doleful accent. 'My gift of writing, such as it was, has left me. I am strong in health, but mentally paralysed.' 'The poison of uraemia,' you answered, 'affects the creative parts of the brain for at least six months.' 'Another three months of empty phrases!' I wailed; and you went away to Scotland, leaving me to continue to write and to burn. But every month the burning diminished, and now I am dedicating *Aphrodite in Aulis* to you."

M. Tripodi (*Il Policlinico*, Sez. Med., February 1st, 1935, p. 111) studied the behaviour of the alkaline reserve in the course of hypoglycaemia produced by injections of insulin in normal and diabetic subjects. He found that the alkaline reserve was lowered in normal subjects and in patients with mild diabetes on the occurrence of hypoglycaemic symptoms, while in severe diabetes with acidosis it rose at first and then fell, but as a rule not below the original level before the administration of insulin. The intravenous injections of large doses of bicarbonate of soda on the appearance of hypoglycaemic symptoms did not prevent the occurrence of more severe symptoms or relieve severe symptoms when they had once developed. Tripodi concludes that the production of hypoglycaemic symptoms caused by administration of insulin cannot be connected with the excessive concentration in the blood of ketonic and lactic acids, as maintained by some writers.

Clinical Memoranda

Acriflavine Bowel Instillation for Relapsing Fever (B. abortus)

This case report should prove of interest from the therapeutic point of view.

A schoolboy, aged 12, during the latter part of a term at school, began to feel out of sorts, and gradually lost 7 to 8 lb. in weight. At home he was thought to be run down, but he began to develop a swinging temperature, eventually rising to 102° in the evening, returning to normal in the morning. Typhoid fever was suspected, but the Widal reaction was negative. However, the agglutination test for *B. abortus* of relapsing fever was found to be positive. Salicylates and quinine seemed to be of no account, but *B. abortus* vaccine improved the condition somewhat, but it may only have been one of the usual intermission periods of a lower degree of swinging temperature. Instillation of half a pint of one five-thousandth neutral acriflavine solution into the colon, repeated daily for one week, brought the temperature speedily to normal, and it has remained so ever since. From that time the boy's condition has improved steadily, and he has now quite recovered from his illness, the duration of which was about three weeks.

Acriflavine is rapidly absorbed, and is being excreted through the kidneys within half an hour; the presumption is that it exercises an antiseptic action on the blood stream.

Prestwick. ARNOLD JONES,

A Case of Intestinal Obstruction

The following case of apparent volvulus with faecal vomiting, the patient living without being operated on, seems worthy of record.

A male East Indian of about 23 years walked into the Mabaruma Hospital, North-West District, British Guiana, at 8.30 one night and reported that a man had struck him on the back with a stick. On examination a small abrasion about the size of a threepenny-piece was seen over the sacral region. No injury to the spinal column could be detected. The bladder was distended. A rubber catheter was passed and left in. The next day the patient had faecal vomiting and looked very ill. The stipendiary magistrate was informed and the patient's dying deposition taken. I may here state that, as a result of inquiries made by the police, they were satisfied that the patient had not been assaulted. Laparotomy was not considered, as most authorities agree that when faecal vomiting occurs it is really to be regarded as a sign of impending death. However, the patient was given several turpentine enemas with no result. A rectal tube was passed, still with no result. The patient was placed in the knee-elbow position and turpentine enemas repeated frequently. On the fourth day after admission coffee-ground stools were obtained after enemas. On the fifth day small amounts of offensive faeces resulted from enemas. During this period the patient was still vomiting offensive faecal matter, the large intestine being distended and no peristalsis being visible. Pain around the umbilicus was complained of, and the patient was very restless. His temperature never exceeded 99.2° F., pulse 100, and respirations 30. On the seventh day after admission the bowels were moving normally, stools being black and very offensive, and the motions frequent and painful. Bloody-looking stools continued for a fortnight. Patient gradually improved, and was discharged as recovered fifty days after admission.

I am indebted to Dr. J. A. Henderson, surgeon-general, for permission to publish these notes.

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Reviews

VITAMIN B REQUIREMENTS

Some ten or twelve years ago Professor GEORGE R. COWGILL set out to determine the *Vitamin B Requirement of Man*,[1] his curiosity being stimulated partly by an observation that the rat seemed to require more vitamin B per gram of body weight than did the pigeon, and partly by a remark made by Professor T. B. Osborne that "man cannot possibly require as much vitamin B per unit of weight as the rat; if such were the case all of us would die early from beri-beri." Cowgill's experiments on this subject have been reported in the literature from time to time. They are summarized and collated in the book under review, and the conclusions drawn from experiments on four species of animals of different maximum body weight (the pigeon, dog, rat, and mouse) form the basis for a calculation of man's requirement of vitamin B. This calculation has been tested by the author, by careful examination of the vitamin B content of human diets in places where cases of beri-beri have been known to occur. Although it must have been exceedingly difficult to make this estimation at all quantitative, yet the approximation arrived at agreed very well with the value forecast by Cowgill as the result of his animal experiments. In the midst of his work came the announcement that vitamin B was not a single factor, but consisted of at least two factors, but as Cowgill's experiments had been based on symptoms (chiefly anorexia) which were later recognized as symptoms of uncomplicated vitamin B_1 deficiency, his early results still held good for the anti-neuritic vitamin. Briefly, Cowgill's method of experimentation was as follows.

He obtained a large quantity of dried brewers' yeast, which he mixed thoroughly and used for all his experiments. He determined the minimum quantity of this yeast which would keep a pigeon at a constant weight. From the figures obtained from twenty-three birds, this amount was evidently a function of the body weight. The values 1/3, 2/3, 3/3, 4/3, up to 8/3 were successively substituted for n in the formula $\frac{\text{vitamin}}{\text{weight}^n} = K$, and the mean value of K for all the pigeons determined in each substitution of n. The average deviation from the mean in each series of figures was found. The smallest deviation was taken to indicate the best value of n. For the pigeon it proved to be 5/3 with a corresponding value of K = 0.0037. For the dog, rat, and mouse also, n proved to be 5/3. The values of K varied, and, ultimately, were found to vary inversely as the maximum normal body weight of the species. Also, regarding $W^{2/3}$ of the formula to represent metabolism, it becomes Vit. = K.Weight.Metabolism, whence, finally, the formula for the vitamin B_1 requirement of man is found to be, in its simplest form,

$$\frac{\text{Vit. i}}{\text{Cal. i}} = 0.0000284 \text{ Weight i (in grams)}$$

(i indicating individual.)

Professor Cowgill will probably put this formula to the test by means of the clinical use which has been made of the international standard for vitamin B_1 (an adsorbate on fuller's earth of vitamin B_1 from a watery extract of rice polishings). It is to be regretted that the only use that Professor Cowgill seems to have made of the international standard for vitamin B_1 is to "assay it by the weight-maintenance technique and by the curative test." Surely this is putting the cart before the horse, and it would have been more valuable to state the results of this experiment simply as proof that the weight-maintenance technique and the curative test measured the same factor. The international standard was not available until about four years ago, but the conversion of his "milligram-equivalent, a unit based on the present studies" into international units would have been a very simple matter, and would have made his book more easily understood in the many laboratories where the international unit is already in use.

Professor Cowgill is, however, to be warmly congratulated on the success of his attempt to put the dosage of vitamin B on a really quantitative basis. His application of mathematical methods to this problem would indeed have surprised the investigators who, only fifty years ago, were beginning to suspect the existence in very minute quantities of "substances other than protein, fat, carbohydrates, and salts, which are necessary for nutrition" (Bunge), and those later investigators who, only twenty-five years ago, were beginning to test these suspicions by actual experiment.

[1] *The Vitamin B Requirement of Man.* By George R. Cowgill, Ph.D. New Haven: Yale University Press; London: H. Milford, Oxford University Press. 1934. (Pp. 261. 18s. net.)

SURGERY OF THE LATERAL VENTRICLES

Dr. WALTER DANDY of Baltimore has followed up his monograph on benign tumours in the third ventricle with a survey[2] of his experience with "benign encapsulated tumours in the lateral ventricles of the brain." It is surprising that even his large practice should have included fifteen of these tumours, which appear to be so rare that no mention is made of them in most reviews of cerebral tumours. Only twenty-five have been recorded in the literature, all, except one, from post-mortem material. Cushing, in his last series of 2,000 tumours, found three papillomas of the choroid plexus in the lateral ventricles. Walshe found no instance in his 642 cases from the National Hospital, Queen Square. In this small series Dr. Dandy's results are even more striking than in the case of tumours of the third ventricle. Apparently the operative approach to the lateral ventricle is simpler than that to the third ventricle, although in several cases one frontal lobe had to be resected at operation. Of his patients three died soon after operation, the remaining twelve being reported as "well" at periods varying from three weeks to fifteen years after operation. The nature of the tumours varied. They were rounded, encapsulated tumours, attached either to the ependyma or to the choroid plexus. The majority appear to have been of ependymal origin: there was one adenoma of the choroid plexus, one venous angioma, and three "fibromas of the choroid plexus." The latter type appears to resemble closely the common "meningioma" or "cerebral endothelioma." In only one case did the clinical symptoms suggest the position of the tumour. In all except one of the others the localization of the tumour depended entirely on the results of ventriculography; in fact, one of these cases was among the first in which ventriculography was used. In one of the fatal cases a cerebellar operation was performed owing to mistaken diagnosis, and a tumour was found protruding into the upper end of the iter of Sylvius; the author feels that if ventriculography had been used in this case the tumour would have been localized and perhaps successfully removed.

Although such tumours represent only a very small proportion of cerebral tumours, perhaps about 1 per cent., their diagnosis and removal have been so successful in the author's hands that they assume a certain importance. His principle that every cerebral tumour should be presumed to be removable until found at operation not to be so, is certainly justified by this series. He further states in his introduction that "there are now no tumours giving signs and symptoms that cannot be accurately diagnosed, precisely localized, and disclosed at operation."

[2] *Benign Encapsulated Tumours in the Lateral Ventricles of the Brain.* By Walter E. Dandy, M.D. London: Baillière, Tindall and Cox. 1934. (Pp. 189; 83 figures, 2 tables. 20s.)

That this should be so is a remarkable result of the advances made during the past twenty years in the surgery of the brain, a result to which ventriculography, devised by the author in 1918, has contributed a large part.

PRINCIPLES OF MEDICAL ELECTRICITY

Written with an engaging directness, Dr. CUMBERBATCH'S *Lectures on Medical Electricity*[3] will hold the attention of even the most desultory of students. The lectures, twelve of them, were planned with a view to overcoming the difficulty of finding time at hospital for both theoretical and practical instruction, and they are intended to be followed by appropriate demonstrations and experiments. These are listed at the end of each chapter.

In its course of little more than two hundred pages the book deals in a surprisingly comprehensive way with the principles of medical electricity. The first lecture is on the galvanic current and its method of action in the treatment of disease, and the next three deal with galvanism, medical ionization, and electro-chemical cauterization. Lectures V to VIII describe the use of electrical currents in stimulating muscle and nerve and in testing their electrical reactions. Students often have difficulty in understanding the significance of the results of electrical testing of muscles, but they will here find the essence of the subject presented with most practical clarity. The remaining lectures describe the physiological properties of diathermy and its uses in medicine and surgery, treatment by currents derived from static electrical machines, concluding with a brief account of infra-red, visible, and ultra-violet rays. Throughout the text are notes for dictation, which summarize the salient points of each lecture. These are printed in smaller type than the main text, and form a useful means of recapitulating the substance of the lectures.

The colloquial manner in which the book is written makes it vivid and entertaining, and, since the author is thoroughly conversant with his subject, it is free from vagueness and inaccuracies. The book is sure to be a success, and it can be possessed for the modest sum of six shillings.

MATHEMATICS FOR STUDENTS OF BIOLOGY

In his preface to the first edition of Dr. W. M. FELDMAN'S *Biomathematics*, the late Sir William Bayliss warmly commended a sincere attempt to explain to intelligent persons untrained in mathematical methods how these methods may be usefully employed in biological, or even clinical, work. The appearance of a second edition[4] proves that the attempt has been successful. Dr. Feldman not only writes clearly but his choice of examples is good; he avoids an irritating triviality on the one hand and a puzzling complexity on the other. The new edition has been carefully revised, and two new chapters, one on nomography, the other on the estimation of errors of observation, added. In his preface to the original edition Sir William Bayliss sounded a faint note of warning regarding the chapter on statistical methods. For somewhat different reasons we regret that the section was not omitted from the new edition. There are many excellent introductions to the study of statistics which those who have mastered the remaining chapters of Dr. Feldman's book will not find hard reading. Dr. Feldman had neither sufficient space nor, perhaps, sufficient interest in the technique to deal adequately with statistical methods; the result is that the chapter is incomplete and not wholly accurate. In many biological applications the analysis of variance which we owe to Professor R. A. Fisher is so important a method of inquiry that an account of it should find a place in any introduction to modern biometry. On the other hand, it is a waste of space to devote eleven pages to an incomplete account of frequency curves and curve fitting. But, as we have said, Dr. Feldman's book is weak where weakness is least detrimental; the writings of Yule, Elderton, and Fisher are familiar to most persons likely to use statistical methods in biology. Dr. Feldman's strength is in his exposition of the grammar of a language a reading knowledge of which *must* be acquired by those who intend to form opinions for themselves upon the most important problems of laboratory, administrative, and even clinical medicine. Nobody expects a medical man now to be an Einstein, any more than two hundred years ago he was expected to be a Cicero. Two hundred years ago he who could not read Latin easily was shut out of intimacy with the best thought of the time. Now, he who has not mastered the notation of mathematics is shut out of intimacy with much of the best thought of our generation. The beginner will find Dr. Feldman an excellent teacher.

FRENCH SURGERY IN THE PAST TWO CENTURIES

Dr. DE FOURMESTRAUX'S account of the makers of French surgery in the eighteenth and nineteenth centuries is very welcome.[5] The author has read widely and to such good purpose that he has been able to throw his work into anecdotal form, giving short biographies of the great French surgeons which sparkle with Gallic wit and are not wholly free from satire. He divides the period into surgery during the Revolution, Directoire, and the Empire; surgery from 1835–46, when general anaesthesia came into use; the introduction of antiseptic surgery; contemporary surgery; the surgery of the war; and the future of surgery. Desault and Bichat, Boyer and Larrey, Dupuytren and Ma'gaigne, Nélaton, Berger and Farabeuf, with many others less known outside their native country, but still great surgeons, are brought to life again. We are told of their friendships, of the schools they founded, and of their quarrels. They hated each other with a wholehearted hatred which was impossible in our colder climate. The hatred of Jesse Foot for John Hunter would in France, at the same period, have been looked upon as a mere disagreement. "Monsieur," said one in the course of an open discussion, "some points in your paper are new, others are good, but, unfortunately, what is new is not good and what is good is not new." Some of the teachers founded schools which appear to us fantastic. Broussais, for instance, and his pupils used eighty million leeches in the year 1834. The supply was exhausted, and messengers had to be sent for more to Hungary and Asia Minor. But there were many really great surgeons like Dupuytren, Larrey, and Nélaton. They were succeeded by others who founded surgery upon a very thorough knowledge of anatomy, whilst others like Malgaigne and Nicaise devoted such leisure as they had to the study of the history of surgery, with very fruitful results. In relation to history it may be noted that Dr. de Fourmestraux quotes verbatim the letter of Littré to Sédillot, which is dated February 28th, 1878. It suggests the word "microbe" to designate animalculae. He says it is short and good Greek, and recommends Sédillot to use, and not to enter into any

[3] *Lectures on Medical Electricity*. By E. P. Cumberbatch, B.M., D.M.R.E., F.R.C.P. London: H. Kimpton. 1934. (Pp. 236; 38 figures. 6s. net.)

[4] *Biomathematics. Being the Principles of Mathematics for Students of Biological Science*. By W. M. Feldman, M.D., B.S., M.R.C.P., F.R.A.S., F.R.S.Ed. Second edition, reset and enlarged. London: Charles Griffin and Co., Ltd. 1935. (Pp. 480; 164 figures. 25s. net.)

[5] *Histoire de la Chirurgie Française (1780–1920)*. By L. de Fourmestraux. With a preface by Professor J. L. Faure. Paris: Masson et Cie. 1934. (Pp. 232. 30 fr.)

controversy about, the term. The change from the old septic surgery to modern methods proceeded in France very much as it did in this country. Lucas-Championnière is given his due meed of praise, for without his advocacy and personal knowledge Listerian surgery would not have been accepted for a long period. Dr. de Fourmestraux has had considerable difficulty with the names of English-speaking surgeons. It is not easy at first sight to recognize "Holmès, O. W." as Oliver Wendell Holmes, or "Wels" as Sir Spencer or Horace Wells. Chislehurst has proved a stumbling-block to him more than once, nor should the author have confused Robert Liston the surgeon who first amputated under anaesthesia with Lord Lister, as he has done on page 87. But these are minor blemishes. The whole book is welcome, and is readable from beginning to end. Professor J. L. Faure contributes a thoughtful preface.

Notes on Books

Dr. ALFRED WORCESTER'S little book on *Sex Hygiene*[6] consists of a series of eleven lectures, delivered between the years 1899 and 1933 to schoolmasters, medical and theological students, and other young men and women on various aspects of sexual hygiene, including prevalence of sexual troubles, sexual dangers in war time, medical service in sex hygiene, social hygiene and the college students, the teaching of sex hygiene, and control of the sexual instinct.

The second annual issue of Post-graduate Clinical Medicine,[7] whose predecessor we noticed last year, contains papers on the following subjects by well-known German authorities: the treatment of premature infants, by Dr. Werner Gottstein (Berlin); post-infective encephalitis, with special reference to cases following vaccination, by Professor H. A. Gins (Berlin); coeliac disease, by Professor Erwin Schiff (Berlin); active immunization against diphtheria, by Professor Hans Schmidt (Marburg); so-called recurrence after operations on the gall-bladder, by Professor B. O. Pribram (Berlin); tuberculosis in children, by Professor M. Klotz (Lübeck); percutaneous therapy, by A. Laqueur (Berlin); Ménière's disease by Professor F. Kobrak (Berlin); endometriosis, by Professor L. Seitz (Frankfurt); the present position of the toxaemias of pregnancy, by Professor R. T. von Jaschke (Giessen); abortion, by Professor S. Hammerschlag (Berlin); Weil's disease, by Professor C. Hegler (Hamburg); protein therapy, by Professor R. Schmidt (Prague); arthritis deformans juvenilis, by Professor E. Schiff (Berlin); the medical aspects of child training, by Dr. W. Gottstein (Berlin); tuberculosis in the aged, by Dr. H. Ulrici (Sommerfeld); diminished responsibility, by Professor P. Schröder (Leipzig); results of bronchography, by Dr. A. Beutel (Prague); low blood pressure and essential hypotonus, by Professor G. Klemperer (Freiburg I.B.); clinical aspects of lipoidosis, by Professor Max Bürger (Bonn); ocular tuberculosis, by Professor L. Heine (Kiel); myositis, by Professor H. Curschmann (Rostock); respiratory insufficiency, by Dr. A. J. Anthony (Hamburg); sport medicine, by Dr. W. Kohlrausch (Berlin); inherited injuries due to *x* rays and radium, by Professor H. Martius (Göttingen); and calcium therapy, by Professor Curschmann.

The little work entitled *Silent Hours*[8] consists of a series of sympathetic sketches of life in a sanatorium, by the well-known Swiss litterateur M. ROBERT DE TRAZ. The psychology of the tuberculous patient in various stages of the disease is admirably portrayed. The book will naturally be of interest to those who have lived in a sanatorium as patients or members of the staff, but it will also appeal to a much wider circle of readers. The translator has done her work well.

Volume liv of the *Transactions of the Ophthalmological Society*[9] is outstanding in respect of the variety of subjects dealt with and the number and excellence of the illustrations. The proceedings of the annual congress in London last April and of the Oxford Ophthalmological Congress occupy the greater part of the book, much of the remainder being devoted to papers read at meetings of the North of England and the Irish Ophthalmological Societies, which are affiliated to the main society. The volume thus contains a large collection of original investigations, expert opinions on many subjects, and practical suggestions for diagnosis and treatment. The communications are classified in groups in the respective contents tables, making reference easy, and there is an index of subjects and authors.

The official *Medical Register* for 1935 has now been published.[10] It appears from the table printed in the introductory pages that 1,664 names were added last year—122 more than in 1933. New registrations numbered 847 in England, 471 in Scotland, 154 in Ireland, and 185 on the Colonial and Foreign lists. The total number of names on the *Register* at the end of the year was 57,496, representing an increase of 755. The number of names removed from the *Register* during 1934 was 959, compared with 962 in the previous year. Of these, 898 were deleted on evidence of death, 54 for failure to comply with the inquiries of the Registrar as to cessation of practice or change of address, and six were struck off under the disciplinary powers conferred by Parliament upon the General Medical Council. The average annual number of penal erasures during the past twenty years was five.

[6] *Sex Hygiene. What to Teach and How to Teach it.* By Alfred Worcester, A.M., M.D., Sc.D. Springfield and Baltimore: Charles C. Thomas; London: Baillière, Tindall and Cox. 1934. (Pp. 134. 11s. 6d.)

[7] *Klinische Fortbildung.* Edited by Professor Dr. Georg Klemperer. 2 Jahresbund. Berlin and Vienna: Urban and Schwarzenberg. 1934. (Pp. 799; 176 figures, 7 coloured plates. RM. 30, paper; RM. 35, bound.)

[8] *Silent Hours.* By Robert de Traz. Translated by Dorothy W. Richardson. London: G. Bell and Sons Ltd. 1934. (Pp. 160. 5s. net.)

[9] *Transactions of the Ophthalmological Society of the United Kingdom.* Vol. liv. Session 1934. London: J. and A. Churchill, Ltd. 1934. (Pp. 632; 122 figures. 30s.)

[10] *The Medical Register, 1935.* Printed and published under the direction of the General Council of Medical Education. London: Constable and Co., Ltd.

Preparations and Appliances

SAFETY VALVES FOR USE WITH ANAESTHETIC APPARATUS

The letter printed below has been received for publication from the Surgical Instrument Manufacturers' Association. It is signed by the chairman, Mr. H. Guy Drew (Down Bros., Ltd.), the vice-chairman, Mr. P. G. Phelps (Mayer and Phelps, Ltd.), the honorary treasurer, Mr. F. C. King (Allen and Hanburys Ltd.), the honorary secretary, Mr. W. T. R. Beckett (Beckett and Bird, Ltd.), and other members of the committee, as follows: Mr. A. W. Down (Down Bros., Ltd.), Dr. A. N. Gardner (J. Gardner and Son), Dr. E. G. Greville (Medical Supply Association, Ltd.), Miss H. E. Hotchkin (John Weiss and Son, Ltd.), Mr. E. W. Mayer (Mayer and Phelps), and Mr. T. M. Proudfoot (Arnold and Sons).

The Surgical Instrument Manufacturers' Association has received a communication from Sir Francis Shipway calling attention to the fact that during anaesthesia induced by a warm ether apparatus a fatality recently occurred, owing to the bellows having been connected to the wrong (or outlet) tube of the ether or chloroform bottle, with the result that liquid anaesthetic was pumped into the patient's throat. He emphasized that something must be done at once to prevent a repetition of such a disaster.

The Committee of the Surgical Instrument Manufacturers' Association are of the opinion that a question of such general importance should come within the province of the Institute of British Surgical Technicians now in course of formation, but in view of the urgency of the matter have taken immediate action and have designed safety valves for use with warm ether apparatus, Junker's chloroform bottles, and

similar anaesthetic units with which such an accident might occur in use.

The valves designed have been submitted to and approved by Sir Francis Shipway, and are shown in the illustrations below. Prints showing the exact nature and method of manufacture of these valves have been circulated to every surgical instrument firm in Great Britain affiliated to the association. Particulars of these valves have also been circulated to the Incorporated Association of Hospital Officers and the Central Bureau of Hospital Information.

In designing these valves the committee have borne in mind that it is necessary to render safe, in the event of incorrect fitting of bellows, the great number of existing chloroform and ether apparatuses now in use, and that this must be effected in a simple, uniform, and *economical* manner.

In investigating this apparently simple problem a certain number of difficulties were encountered. Many varieties of valves were tried (glass-to-glass, etc.) and were found unsatisfactory. Some satisfactory valves were rejected as expensive and complicated.

Valves for use with Liquid Chloroform.—The ordinary Rigby type ball valve, fitted to the inlet tube, when immersed in chloroform was found to be sufficiently reliable to be considered safe. A ball valve fitted to the outlet tube (dry and air-exposed) is not uniform in its action, is not so effective, and is not considered safe.

Valves for use with Liquid Ether.—In this case conditions are very different from those when using chloroform. Ether is extremely mobile and penetrating. It has a low specific gravity (about half that of chloroform), and is a non-viscous fluid. Immersing a ball valve in ether has little effect, and does not improve the sealing and functioning of the valve to the extent as when it is immersed in chloroform. Experiments show that a simple ball valve fitted into the inlet tube immersed in ether, unless it is a super-perfect valve, cannot by itself be considered safe. A ball valve fitted to the outlet tube has the effect of making the anaesthetic bottle a closed chamber. The ether acting under the pressure of its own vapour enters the inlet tube. The method of a single ball valve on the outlet tube cannot by itself be considered safe. It is found when using a spring-loaded ball valve on the inlet tube that if a small hole (about 1.5 mm. diameter) is drilled at the top of the inlet tube inside the bottle it is sufficient to ensure the safe working of the ball valve. On trial it is shown to prevent the ejection of ether even when the ball valve itself is not perfect.

In drawing up the recommendations noted below the committee recognize that ball valves are not always perfect, and at times may leak. In the case of chloroform the fluid is heavy and is sufficiently viscous to more or less seal any slight defect. When using ether conditions are entirely different; there is practically no sealing.

The recommendations are as follows:

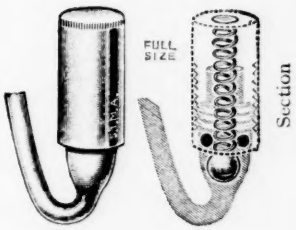
All Chloroform Bottles to be fitted with a ball valve of the Rigby type, spring-loaded, as shown on the prints and illustrations circulated. The inlet tube at the point where the bellows are attached should be plainly marked with a "B" or in some other unmistakable manner.

Ether Bottles, Shipway's Pattern.—Inlet tubes to be marked as noted for chloroform bottles. The inner tube should have a 1.5 mm. aperture at the top, inside the bottle. This aperture should be made in such a way that it is not possible for it to become accidentally covered by the rubber bung or extending tube. The distal end of the tube which dips into the ether should be fitted with a ball valve of the Rigby type. The ball should be fixed in such a way that it cannot be accidentally displaced. It should be held down against its seating by a spring contained in the cap. This spring should be fixed to the cap or rendered inaccessible in some way so that it cannot be accidentally lost or displaced. The exit tube should be furnished with a well-made valve, such as a Rigby ball valve with a spring-loading (made with the same precautions as before stated) or a flat ground copper plate valve (Major L. N. Routh's model) with a spring-loading.

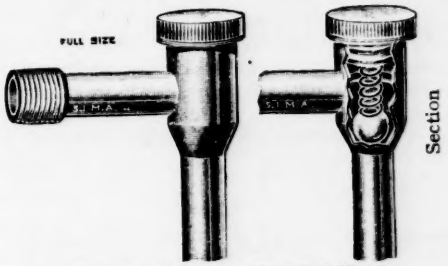
When using the above valves one point must be borne in mind. Careful examination of safety valves which have been returned for repair after prolonged use show that these valves collect sulphur and chalk from the rubber bellows, and fluff and cotton-wool from the air stream. These materials become deposited in the valve; it is therefore necessary that valves should be tested periodically, and if found not to be functioning satisfactorily they should be returned to the makers for adjustment.

General Note.—The committee recommend all members of the Surgical Instrument Manufacturers' Association neither to stock nor list any anaesthetic apparatus with which it is possible for liquid anaesthetic to be injected into the patient's throat. Further, that if asked to make an apparatus with which such an accident is possible, they should on every occasion warn the intending purchaser of its potential dangers.

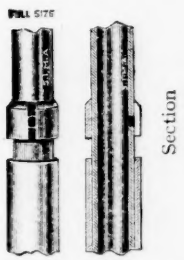
Shipway's (S.I.M.A.) warm ether apparatus bottle fitted with valves.



Spring-loaded (S.I.M.A.) Rigby's valve.

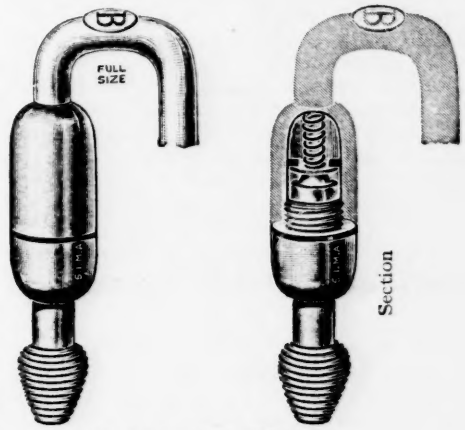


Spring-loaded exit valve (S.I.M.A.).



Perforated inlet tube (S.I.M.A.).

* For use as an exit valve the position of the inlet and outlet tubes should be as on the spring-loaded exit valve in the other illustration.



Routh's inlet valve* (S.I.M.A.).

CANCER AND CAUSATION*

BY

G. W. NICHOLSON, M.A., M.D.

PROFESSOR OF MORBID ANATOMY, UNIVERSITY OF LONDON, GUY'S HOSPITAL

It is surely a good sign that Mr. A. H. B. Kirkman[5] should express the profound dissatisfaction of a large majority of the medical profession with the mass of contradiction and the confusion of ideas that represent the cancer problem, so-called. It is only natural that cancer research should believe that it understands the problem and is on the way to giving the solution, whereas biology, if asked, might disagree most emphatically. That biology has the final say in the matter ought not to surprise, for what is cancer if not a manifestation of life and, therefore, a strictly and entirely biological phenomenon, as inevitable and *natural* in certain conditions as embryonic development and physiological form and function in certain others? But biology shows little or no interest as yet, and we must shift for ourselves as best we can. This is my excuse for stating my opinions. I speak for myself, of course.

MALIGNANCY

As biology distinguishes between genesis of the organism and of every organ and tissue, and subsequent differentiation of the physiological characters of each, so should we distinguish between causation and birth of a cancer, and subsequent development of its proper characters. Of these malignancy is the most conspicuous and important for us. I have nothing to say of malignancy except this. It is clearly a physical sign and symptom of that form or upset of metabolism which is known as cancer, and in no sense of the word a "cause"; it is a problem of growth like that of all forms of physiological growth; and it is too much to expect that it will be solved before these by scientific methods. But all this is really of minor importance for the medical profession, for, prevent formation of cancer, and malignancy will automatically cease to trouble. Our most urgent concern is with causation.

CAUSATION IN THE LIVING ORGANISM

As we ask, so shall we be answered; and even the most scientific methods of observation and experiment scientifically used will not answer unscientific questions. We are, somewhat impatiently, clamouring to be told the "cause" of cancer, but forget that biology does not pretend to explain the "cause" of life. Are we not asking the wrong question, or, maybe, the right question in the wrong form? Do we not perhaps understand the principles of tumour formation better than we know?

Clearly our first concern is to understand the meaning of the word causation in the living organism. The *cause* of a phenomenon is, of course, the factor, or factors, necessary for its being. Now, every somatic character whatsoever ("physiological" and "pathological") is acquired as a *reaction* of the organism to a *stimulus*. Nothing happens without both: the stimulus *and* the reaction, I repeat. Both are equally necessary for the existence of the character, but not therefore for the form—the essential nature—of the character. This can be illustrated better by a homely example than by pages of philosophical argument: If I kick a log, I stub my foot; if I apply the same kick to a small child, he howls; if to a man bigger than myself, he knocks me down—or, should he be a bigger bully than I, runs away. Again, should I tender a coin instead of a kick, the response will be another in all, though still varying from case to case. This means that the same stimulus is followed by very different effects. The stimulus simply *in*duces a response. But the form of the response or reaction is *pro*duced by the object stimulated: in scientific language, by the constitution—that is to say, the physico-chemical composition—of the organism. Not only will the same stimulus induce different reactions in different individuals, but in the same individual at different times (provided his constitution be modified by interim reaction). And a moment's thought will show that *diverse stimuli will induce the same reaction.*

Perhaps the most important points are these: We cannot predict the form of the reaction except from previous knowledge of the organism; it is safe to kick a known coward, but not a stranger of equal bulk. Again, why diverse stimuli of totally different origin and composition induce the same reaction, whereas others, though closely related, do not, is indeed a mystery of life: mercifully it is no more the concern of cancer research than of biology, which, wisely, never asks the question "Why?" *That* they induce it is simply a matter of experience. *How* they induce it can only be explained, with present knowledge, with the not very satisfying statement that the organism reacts in the only way it can, the way it must, the way it is made. The biological sciences are in no better case.

CAUSATION OF CANCER

It follows of logical and biological necessity that the *cause* of cancer is a reaction of the organism to stimulation, and this regardless of whether the stimulus be always the same or whether it be one of many interchangeable stimuli, as it appears to us. As everywhere in biology, the "cause" is not so much the nature and composition of the stimulus or stimuli as the nature and composition of the reacting organism. When cancer is seen as a biological phenomenon, the principle of its causation is as clear and distinct as that of every other manifestation of life.

To the best of my knowledge Morley Roberts[6] was the first to illuminate pathology by applying the biological conception of causation to cancer fifteen years ago, when he says: "If it is then seen that there is no invariable single antecedent to malignancy, it must be admitted that there are many exciting 'causes,' of which *x* rays are but one. If such is the case, it follows logically that it is in the tissues concerned, their nature and relationship, that the true cause must be found." He thus shows cancer research how to take "a definite view of malignancy as a natural disorder and one only too likely to occur in such a construction as the mammalian body."[6] He has developed this biological conception of cancer.[7] Small wonder that five of those who see with him that the living body is really and truly alive should mark their appreciation of his insight.

ACCEPTED VIEWS

Cancer research, however, when not obsessed with the notion that cancer is "caused" by constitution alone (for example, a chromosomatic mutation or malformation), persists in the vain—since philosophically unthinkable—idea that the composition of the stimulus accounts for the nature of the reaction and therefore the character. Naturally failing to "explain" how diverse stimuli "produce" one effect, it too often hopes to find one "cause" behind—that is to say, instead of them all, conceived as a necessarily invisible "virus" for want of a better. Although no reasonable human being will deny that one or more forms of cancer may easily be induced by one or more ultramicroscopic living organisms, to extend the notion to all cancers is, with present knowledge, an offence against the principle of economy of hypothesis, based upon a logical fallacy.

KNOWN STIMULI

It is hard to get away from the fact that chemical compounds of diverse composition and origin, *x* rays and other forms of radiant energy without "chemical composition" at all, visible animal parasites, etc., are as surely followed by cancer as infancy by old age. Experimental pathology can claim with just pride that for tar cancer at all events it has demonstrated the steps in formation and subsequent development of a malignant tumour with the same degree of precision and complete-

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ness and with the same method as embryology for the liver (see Deelman[3]). When allowance is made for differences in individual susceptibility—that is, constitution, reduced to a minimum in "pure strains"—pathology can assert, with experimental embryology, that the result of applying tar to the skin of a mouse "is just as definitely causally determined and predictable as the result of mixing two known reagents in a test-tube," if I may quote Huxley and de Beer.[4] Verily, the "cause" of tar cancer is as fully known and understood as the origin and development of the liver. Nay, better: for is not pathology immeasurably favoured above embryology, and cannot we see, smell, handle, weigh, and analyse the stimulus by the ton? And cannot we reproduce the cancer at will?

Moreover, we owe to Kennaway and his school[2] exact knowledge of the chemical composition of the "carcinogenic agent" in tar, and are thus able to think in terms of "exact" science. More important still, he[1] has quite recently told us the structure of another active compound obtained by simple chemical means from bile, thereby establishing "a direct relationship between the carcinogenic compounds and some normal constituents of the body," and opening up vistas of endless possibilities.

CONCLUSION

Thanks to cancer research, our knowledge of stimuli that induce cancer, though very far from complete, is sound, more extensive than we are prepared to see, and increasing apace. Knowledge of the reaction, since unlooked for, is all to come. And knowledge of the changes in physico-chemical composition of the organism, the very essence of cancer, is naturally no greater in pathology than in physiology, embryology, and biochemistry. All would be well if cancer, like an infection, were induced by a single stimulus, and few would suspect that they know and understand no more than by far the easier half of the "cause." But cancer happens to be induced by diverse, interchangeable stimuli. Hence the confusion, when biological principles are ignored and we forget that we are alive. But to say with those who, knowing least, shout loudest, that cancer is veiled in mystery—mystery, mind you, deeper than that surrounding biology, every simple physiological reaction and every fully "explained" pathological lesion—is, frankly, to say the thing that is not.

SUMMARY

Cancer is a biological problem. Like every biological problem it is a philosophical problem too, not to be solved with a logical fallacy.

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VOLUNTARY BLOOD DONORS ASSOCIATION

REUNION DINNER

The second annual dinner of the Voluntary Blood Donors Association took place in London on March 21st, when two hundred members and their friends assembled under the chairmanship of Lord Horder.

Lieutenant-General Sir Harold Fawcus read greetings from the patron, the Duke of York, and the president, Sir Arthur Stanley. Mr. Geoffrey Keynes, in proposing "The Service," recalled his experiences of blood transfusion in the war, when he was among the first to take advantage of the new technique. Applying it to a number of men in what was known as the "moribund tent" at the casualty clearing station, he was astonished to find how many lives were saved. Some difficulty was experienced in finding donors until fourteen days' leave was attached to this service, whereupon "the whole British Army volunteered."

Mr. P. L. Oliver, head of the British Red Cross Transfusion Service, was also reminiscent. The service began in November, 1921, on the request of the secretary of King's College Hospital for a blood donor, and it had grown until, in 1934, the number of calls in London was 3,855, and already in 1935, although not three months had passed, there had been over a thousand calls. In a recent week the number of calls had averaged fifteen a day. The number of donors on the roll was 1,881, and, including in the computation the new recruits, each of them had given on the average seven transfusions. One donor had given fifty-six transfusions, and had been the means of saving forty lives. Mr. Oliver urged that in future every patient entering the hospital for whom there was the slightest possibility of this service being required should be grouped as a matter of routine, in the same way as temperature and pulse rate were taken. That would save these many midnight calls for a universal donor on the ground that there had not been opportunity to group the patient. The suggestion had, indeed, been made that everybody should be grouped at birth, the grouping being tattooed on some inconspicuous part of the body. It should certainly be made a regulation at the infant welfare centres throughout the country that every expectant mother should be grouped, and, if possible, the husband or a relative should also be grouped in view of an emergency.

The toast of "The Visitors" was proposed by Mr. D. P. Kennedy, deputy chairman of the association, and responded to by Professor J. W. Eyre; and Dr. A. M. A. Moore proposed "The Ladies," a toast acknowledged by Sister Linstead, the first donor of the service. Dr. H. F. Brewer, medical officer of the service, mentioned a recent observation of 300 cases which had shown that, provided the donor did not serve more than once in three months, no harmful result occurred. About 20 per cent. of the donors had told him they had improved in health as a result of the service. An important step forward was the placing on the market of efficient grouping serums at a cheap rate. In reply to the toast of his health, which was proposed by Dr. R. G. Canti, Lord Horder praised this highly altruistic public service. He had made a calculation that, supposing the average amount of blood given by the donors at each transfusion to be 15 oz., no fewer than 323 gallons had been given during the past year. He thanked the donors for a service which had been done to medicine as well as to the patients concerned.

ERADICATION OF THE BED-BUG

At the request of the Ministry of Health the Medical Research Council has undertaken to promote further investigations into the health problem caused by the infestation of houses by the bed-bug, and has appointed the following special committee to advise and assist in this work:

Professor J. C. G. Ledingham, F.R.S., director of the Lister Institute, chairman; Professor P. A. Buxton, London School of Hygiene and Tropical Medicine; Mr. C. S. Elton, University of Oxford; Mr. C. R. Kerwood, Ministry of Health; Dr. John Macmillan, medical officer of health, Woolwich; Dr. G. W. Monier-Williams, Ministry of Health; Professor J. W. Munro, Imperial College of Science; Dr. P. G. Stock, Ministry of Health; Dr. R. E. Stradling, Director of Building Research; Mr. A. W. McKenny Hughes, British Museum (Natural History), secretary.

On the recommendation of this committee grants have been made by the council for research under the direction of Professor Buxton into the natural history of the bed-bug, with reference to the conditions of its viability, and for research under the direction of Professor Munro into chemical methods for its destruction.

British Medical Journal

SATURDAY, MARCH 30th, 1935

SIR GEORGE NEWMAN

The retirement at the end of this month of Sir George Newman from the posts of Chief Medical Officer to the Ministry of Health and to the Board of Education is an event of much importance both to the medical profession and to the nation. His tenure of these posts has covered a period of great development in the public health activities of the State. In the year in which he was appointed to his position at the Board of Education was passed the Education (Administrative Provisions) Act, which made it the duty of local education authorities to provide for the medical care of school children, and a few years later came the Mental Deficiency Act and the Epileptic and Defective Children Act, which provided correspondingly for the care of those whose mental condition required attention. The national health insurance system came into operation at about the same time, and later there have been momentous developments in connexion with maternity and infant welfare, with dental attention, with Poor Law administration, with hospital provision and usage, and with the public health services in general. It is not too much to say that the whole health machine, as we know it to-day, has been organized and perfected in its medical working under Sir George Newman's influence and supervision. His official association with these two great Government Departments has thus been marked by enormous activity, amounting, indeed, to a revolution in health and educational advancement. That these manifold changes, so far as they require the co-operation of the medical profession, have now been got to work with such smoothness, efficiency, and beneficial effect, is a fact which redounds to Sir George Newman's credit, and for which both the medical profession and the whole community owe him profound gratitude.

Before he entered the Civil Service Sir George Newman's career had already been distinguished. As a teacher, as an author, and as an active and zealous medical officer of health he was widely known and respected. He came into the Civil Service from outside with this established reputation ; and it may be that this circumstance prevented him from accommodating himself completely to all the traditions of that Service which are so important and valuable, and from acquiring in full all those characteristics and virtues for which the higher branches of that Service are so justly famed. He may be said to be primarily a medical man, and a supremely effective health propagandist. Lord Riddell once described him, in one of his vivid phrases, as "the most important hygienist since Moses," and he has certainly shown that he possesses in a high degree the faculty of interesting and informing wide circles of auditors and readers on public health matters, and of stimulating a desire for increased knowledge and skilfully directed research. His later, but, we may hope, by no means his last, books exhibit this faculty in a marked degree ; and there can scarcely ever have been any series of official reports which have been so widely read, so inspiring, or so generally valuable as those issued year by year under the titles "The Health of the School Child" and "On the State of the Public Health." Any possible loss arising from a less direct and extensive contact between the Chief Medical Officer and the lay administrators than a more complete civil servant might have established, has been abundantly compensated for by his wide influence on popular thought, by his great authority with medical colleagues in the public health services, and by the confidence he has gained from the medical profession as a whole.

This confidence, which cannot be cultivated except by continuous contact with the practice, the thought, and the opinion of the profession in general, is of great importance to the smooth and effective working of any of the public health services. It is not merely medical officers of health, school medical officers, and their staffs who should be regarded as members of the public health services, but the whole of the profession in whatever branch of practice they may be working. It is upon them, and especially perhaps upon general practitioners, that the safeguarding of the public health depends. It is possible for health departments, both central and local, to alienate them, or by free close and sympathetic intercourse and consideration to enlist their services whole-heartedly and zealously. Sir George Newman has shown throughout that he realizes this situation and shares these views. He has never failed to appreciate the position, the importance, and the value of the general practitioner, or to hold before him a high ideal of character and of service. It is largely by this means that there has been established the present cordial contact and understanding between the organized profession, the Ministry of Health, and the Board of Education, and, indeed, most local authorities also. This is not the least of the many services which Sir George Newman has helped to render.

The medical profession, his own, share with the Minister of Health and the President of the Board of Education the great regret which they have expressed on the occasion of Sir George Newman's retirement. Though the period of his very distinguished official connexion with public administration thus terminates, there can be every expectation that his public services are by no means at an end. His colleagues in the profession will still look forward to his guidance on important matters, and the public to further illuminating exposition of health questions. Sir George may be assured of the cordial appreciation of both for the work he has done and their best wishes for his leisure.

MATERNAL MORTALITY

Recent correspondence in our columns has again testified to the great divergence of opinion in the medical profession regarding the causes of the persistently high maternal death rates which obtain in these islands and the remedy therefor. Some believe that the failure of the widespread efforts towards improvement which have been made during recent years is to be laid at the door of factors of an economic and social nature. Supporters of this view point to the high average rates in many industrial centres. In the towns of Yorkshire, Lancashire, and Wales, for example, it is maintained that the high risk often found to be associated with child-bearing and childbirth is largely attributable to industrial depression and the associated overcrowding and inadequate nutrition. On the other hand, there are those who contend that the maternity record of an area is determined essentially by the nature of the midwifery service with which the area is provided. They point out that there is ample evidence that overcrowding and poverty do not militate against safe midwifery so long as the personnel and the material of the service are adequate. Echoes of both these views were to be heard in the House of Commons this week during the debate on the social services, to which reference is made in our Parliamentary Notes.

In a recent issue of the *Journal* (February 16th, p. 304) Dr. Oxley and Professors Phillips and Young directed attention to the obstetric experience of Rochdale, which is of some interest in this connexion. This Lancashire town of 90,000 inhabitants for many years was in the unenviable position of having one of the highest average maternal death rates in England and Wales. Through the initiative of its medical officer of health a determined effort to remove the reproach was launched in 1931. This was so successful that over the three following years the average death rate was reduced by about 60 per cent. (from 10 per 1,000 during 1929–31 to 3.9 per 1,000 during 1932–4). The writers of this record state that this remarkable result was achieved only after it had become clear from a study of the circumstances leading up to each fatality that the large sacrifice of maternal lives in Rochdale during the preceding years was due chiefly to too frequent intervention in the course of labour by the medical attendants. One of the most important lessons to be drawn from this record is that when the true nature of the obstetric problem of Rochdale was brought to the notice of the local practitioners their response to the appeal was prompt and satisfactory. The local Division of the British Medical Association contributed to this result in an important measure by convening meetings of the doctors at which the facts were frankly and freely discussed. According to Oxley, Phillips, and Young the improvement in the service was due in the main to the exercise of greater care on the part of the doctor and to the fact that difficult cases, instead of being undertaken in the unsuitable environment of a private house, where proper nursing and assistance were difficult to obtain, were sent to hospital. It is significant in this connexion that during the years 1929–31 in a total of 3,691 deliveries there were twenty-one deaths from puerperal sepsis, operative shock, and post-partum haemorrhage, while during 1932–4 there were only nine deaths from these causes in 3,335 deliveries.

It is worthy of note that the practitioners of Rochdale are permitted to treat their patients after their admission to the hospital, which is under the auspices of the local authority. This is a policy which the British Medical Association has for long advocated, and we hope that the advantages which have been so clearly demonstrated in Rochdale may persuade other authorities to follow the lead given there. Oxley, Phillips, and Young believe that the example set by Rochdale is of great value in pointing to the benefits to be expected from a regional attack on the general problem of maternal mortality. They suggest that the institution by each local authority of a frank and periodic review of the circumstances leading to the deaths of mothers in their area would provide information regarding any defects which exist in the local maternity services. They believe that with the knowledge thus gained and with the willing co-operation of all concerned the most effective means would be found for devising measures for dealing with the serious position of the maternity records of many areas in this country.

A point that will not be overlooked by readers who have studied closely the Rochdale figures is that the mortality rates during the period of inquiry showed some tendency to rise after the sharp fall in 1932. The very energetic propaganda instituted through such agencies as the press, the pulpit, and the platform had an immediate effect. Whereas in 1930 the deaths ascribed to pregnancy and child-bearing showed a rate per 1,000 live births registered of 9.63, in 1932 the rate was 1.76. In 1933 it was 2.87, and in 1934 (uncorrected figures only available) it was 4.30. To some minds this will suggest the possibility of a trailing off in the results of active propaganda. None the less, all will agree that Rochdale deserves every credit for its effort and for the large measure of success achieved. Incidentally the results show how much can be accomplished in the reduction of maternal mortality with the powers which local authorities already possess.

INTRAVENOUS OLIVE OIL EMULSION

Accounts having appeared in the lay press last week of a new method of treating pneumonia by injections of an emulsion of olive oil, it will be of interest to medical readers to receive an account of this work from a professional source. It may be remembered that Drs. V. G. Walsh and A. C. Frazer of the Physiology Department of St. Mary's Hospital published last year in these columns[1] a record of experiments which showed

[1] *British Medical Journal*, 1934, i, 424.

that finely divided oil emulsions inactivate diphtheria toxin, apparently by adsorption. In subsequent correspondence and a paper by Dr. G. N. Myers,[1] who had independently made similar observations, there was some discussion about priority. It was suggested by Walsh and Frazer that their discovery might find a practical application in the administration of vaccines or other reagents inducing active immunization, and a case was described in which tuberculin was so administered ; this patient also received intravenous injections of an olive oil emulsion, but the effect was temporary, and the authors stated emphatically that the case was not reported as an example of therapeutic success from this proceeding. No indication that olive oil emulsions were being administered by intravenous injection for therapeutic purposes was given until the appearance of the paper on which recent accounts in the daily press were based. This paper is contained in a book entitled *Technical Aspects of Emulsions* (London: A. Harvey, 1935), which records the proceedings of a conference held under the auspices of the British Section of the International Society of Leather Trades' Chemists. The other papers in this volume are concerned with purely industrial and engineering matters, and "the use of highly dispersed emulsions in the treatment of toxaemic conditions" rubs shoulders with the design of emulsifying machines and problems connected with textiles, leather, rubber and asphalt. In this paper Walsh and Frazer describe the preparation and mode of administration of an olive oil emulsion with which they have successfully treated streptococcal septicaemia, erysipelas, bronchopneumonia, acute rheumatism, and lobar pneumonia. One case of pneumonia is described in which the treatment apparently had a dramatic effect, and other cases of this disease are said to have left hospital in under twenty days practically fit for work ; apart from these statements no clinical records are given and there are no figures of the number of cases treated. It may appear unfortunate that information about this method of treatment should have been communicated to a body of laymen before the medical profession has had any opportunity of forming its own opinion. When this opportunity is given, it will be necessary to consider how far Walsh and Frazer's previous experiments with toxin are applicable to the conditions obtaining in the acute infections which they have since treated. Pneumonia is not a toxaemia in the sense of these experiments, and if an emulsion of oil has the effect claimed for it in this disease it will clearly be necessary to inquire closely into the mechanism of this effect, which may well be of quite a different nature from that postulated. Unless the method is to be accepted on overwhelming clinical evidence and on an empirical basis, it is one which calls for experimental investigation by methods which do not appear yet to have been applied to it and (in as far as clinical employment is justified) for a careful study of its effect not only on fever and the duration of the disease, but on every aspect of the disease process—biochemical, cytological, or bacteriological. We would add that the present is a particularly unhappy time for claims to be made on behalf of methods of medical treatment which await full substantiation.

[1] *British Medical Journal*, 1934, i, 945.

MORE MILK

The peculiar value of milk in nutrition has now been established beyond all doubt, and most school children are able to obtain an extra milk ration with very little difficulty, even if they cannot afford the modest sum asked for its purchase. This position of the pre-school child has long been a source of anxiety to those in authority, and another appeal on its behalf has now been issued by the Children's Minimum Organizing Committee. This body has submitted a memorandum to the Milk Marketing Board, urging the supply of cheap milk, at 1½d. a pint or free, for children below school age and for expectant and nursing mothers. It suggests that maternity and child welfare clinics would form useful distribution centres, with the assistance of day nurseries, nursery schools, and toddlers' clinics: where this organization was inadequate, a shopkeeper might possibly be paid a lump sum to act as a distributing agent. The committee does not regard a third of a pint as an adequate daily allowance, and urges that every encouragement should be given to consume larger quantities. The British Medical Association Committee on Nutrition recommended a pint of milk a day as the basal primary requirement in the diet of children between 1 and 5. The Children's Minimum Committee asserts that many children in poorer families never taste fresh milk : an investigation in Cardiff in 1933 showed that 26 per cent. of poor working-class households had no liquid milk at all. The inquiry also revealed that the consumption per head was much lower in families with children than in those without. When money is short, milk is regarded as an "extra." The committee confidently believes that this insufficiency of milk is a large factor in the continued influx into schools of entrants suffering from impaired health and physique. If half the children under 5 and the expectant and nursing mothers in England and Wales in the "elementary school" stratum of society were to take a third of a pint of milk a day, the total consumption would amount to 26,000,000 gallons a year.

PAPULAR URTICARIA

No skin disorder in childhood causes so much minor discomfort to patient and parent as that known variously as lichen urticatus, papular urticaria, strophulus, gum-rash, heat-rash, "hives," or red-gum. Dr. B. C. Tate has recently, in the *Archives of Disease in Childhood* (February, 1935, p. 27) made an important contribution to the study of this disease, which, while not suggesting any startlingly new therapeutic measure, helps to clarify the whole subject of the complicated aetiology. He starts by asserting, as a result of certain experiments, that lichen urticatus is essentially a different reaction from true urticaria, especially as regards the formation of papules. The disorder is particularly manifest during the first four years of life, is more common in the summer months, and can almost always be "cured" by the admission of patients into hospital, as Tilbury Fox and Hallam observed many years ago. This last observation has been commonly held to suggest an environmental factor, since the rash does not occur in institutions

even if the home diet is imitated. Dr. Tate shows that the influence of heat may be of importance in this respect, because in a carefully planned experiment in which susceptible patients were exposed to different temperatures in special rooms it was clear that a minimum of 68° F. was necessary before the rash appeared. Other factors also enter into the problem, but no one special article of food could be wholeheartedly incriminated. Even when patients admitted to hospital were kept in temperatures above the minimum referred to, and were given diets containing various articles alleged to produce the rash at home, in no single instance did fresh lesions develop during the course of the experiments. In further efforts to determine the home-environmental conditions concerned in the production of the rash Dr. Tate brought into contact with the patients in hospital such articles as bedding from home, house dust, moulds, scrapings from walls and ceilings, and animal size. The results were largely negative or inconclusive. In a small number of instances a crop of lesions occurred after a child slept in hospital on home bedding or after putting on his usual clothes, but Dr. Tate is cautious in attributing too much to such evidence. He sums up by saying that while some external influence is necessary to provoke the rash it is not bedding or clothes, but it can be carried on these articles. He is certain that this agent is not vermin or an antigen derived from human beings or domestic animals. His conclusions on diet, while excluding a specific factor as already mentioned, seem to indicate that excessive carbohydrate feeding may play some part. Digestive disorders, teething, and mild febrile disturbances probably predispose to the condition. Blood examination, as regards both haematology and chemical investigation, failed to reveal any definite departures from normal. The treatment of the condition includes a restriction in carbohydrates, the use of rhubarb and soda internally, and admission to hospital as a certain cure for intractable cases. Local applications are used to deal symptomatically with the itching and secondary changes.

A CARCINOGENIC METHOD OF DIAGNOSIS

The development of medical science seems lately to have involved the introduction of somewhat drastic proceedings in diagnosis as well as in treatment. One of these is the use of thorotrast, a suspension of thorium dioxide which when injected intravenously is taken up, like other particulate foreign material, by reticulo-endothelial cells, and as such cells are congregated largely in the liver and spleen, and thorium dioxide is radio-opaque, it causes these organs to cast radiographic shadows. This proceeding is said to be of value in determining whether the liver contains secondary deposits of growth, in deciding the position of a single large lesion in the liver, such as a hydatid cyst, and in determining whether an abdominal tumour is or is not the spleen. Although ill effects are said not to follow, the prospect of retaining such material indefinitely in these organs is not alluring, particularly when it is remembered that thorium dioxide is radioactive. Prolonged radio-activity is known to result in malignant disease; not only has this been shown experimentally by burying glass tubes containing minute amounts of radium in the tissues of rabbits, but it has twice been recorded in man—once in consequence of the loss of a radium needle inserted for treatment, and in several cases following the swallowing of small amounts of luminous paint by factory workers. There is thus at least a possibility that the diagnostic use of thorotrast may eventually give rise to malignant disease. That it can do so at least under certain conditions has now been proved by Roussy, Oberling, and Guérin,[1] who gave intraperitoneal injections of thorotrast to fifty rats. Of fifteen of these which survived ten months or more eight developed sarcomata; subcutaneous injection in five animals was followed in four of them by the same result. These authors go so far as to recommend injections of thorotrast as a convenient method of producing malignant disease for experimental purposes. Unfortunately no comparison is made between the dose employed and that necessary for radiological diagnosis in man. Nevertheless the reader is left with a strong impression that a laparotomy would be preferable to a proceeding which can in any circumstances have such disastrous effects.

THE PERILOUS THRESHOLD OF LIFE

Just as a new country invariably takes toll from immigrants, so each new generation has to pay the price of its adaptation in infant mortality. It is therefore encouraging to know that there has been a reduction of infant deaths by a half or two-thirds within a single generation in many countries. Part of the amelioration is undoubtedly due to sanitation, but not all. Dr. K. Stouman, in an exhaustive review[2] of the demographic state of infant mortality throughout the world, suggests that the crisis through which we are passing may be one of adaptation, the favourable results of which are already apparent in the reduced loss of life in the early years. The reduction of infant mortality has run parallel with an unprecedented fall in the birth rate. The new generation is produced, and an increase of population maintained, with a far smaller effort than before. This results in an amelioration of the social position of women and an important diminution of the dead charges upon the family and the community. A century ago France and Ireland were the only countries with a low birth rate, and the highest figures were found in Central Europe, but in 1932 Central, Northern, and Western Europe all showed remarkably low figures, and other countries had hitherto unheard-of low rates. On the whole, urban birth rates were lower than rural rates, but there was a decrease in both. In most countries, as is often pointed out, the fall has been heaviest among the fairly well-to-do, but there is some evidence that in Stockholm it is now as great among the poorer classes. An important and rather neglected feature is the diminution in the age of mothers: births late in life are becoming rare; at the same time the birth rate is actually increasing for very young mothers. The result of this change is notably to quicken the succession of the generations, and it is, Dr. Stouman thinks, also likely to have an effect on the infant mortality. The decrease in the general death rate has been brought about largely by a saving of infant life and reduction of

[1] *Bull. de l'Acad. de Méd.*, 1934, No. 41, cxii, 809.
[2] *League of Nations Quarterly Bulletin of the Health Organization*, December, 1934, iii, 531.

epidemic diseases ; little has been done for the chronic affections of more advanced life. Nevertheless, an improved state of health for parents has contributed to the saving of infants. The reduction of mortality is not confined to infancy, but extends to the whole of early childhood. Dr. Stouman concludes that there is no relation between the decrease of infant mortality and the decrease of the birth rate. The birth rate fell steadily for many years in several countries without producing any change in infant mortality. Statistics do not, he finds, give any clue to the probable infant mortality in a large twentieth century family under twentieth century conditions. Discussing the causes of infant deaths in the various countries, he points out that mortality is the heaviest at the time of birth and shortly after, and then rapidly decreases. The continued high mortality in the early days shows that the forces active in the reduction of infant mortality have only benefited the infants after the first shock of birth has been passed. A very early child-bearing seems to be particularly harmful to the infant, and the figures suggest the desirability of allowing two years for recuperation between pregnancies ; a second pregnancy while the first child is still an infant seems to be detrimental to its health. Many towns show exceptionally low infant mortalities because of the better amenities available in urban than in rural areas ; the lowest on record is that of Oslo, where in 1933 only twenty-seven infants died out of every 1,000 live births. This rate is closely followed by those of Amsterdam, Rotterdam, The Hague, Stockholm, Zurich, and certain New Zealand, Australian, and Canadian towns. Elsewhere in the world very few large towns have a low infant death rate, and the mortality in Latin American and Asiatic towns is high, with few exceptions.

INTERNATIONAL VITAMIN STANDARDS

The international standards for vitamins A, B_1, C, and D are now available for issue to laboratories, institutions, and research workers in Great Britain and Northern Ireland. These standards were accepted at the Second International Conference on Vitamin Standardization held in London in June, 1934, under the auspices of the Permanent Commission on Biological Standardization of the Health Organization of the League of Nations, and it was recommended that they should be kept at the National Institute for Medical Research, Hampstead, which would act for this purpose as central laboratory on behalf of the Health Organization of the League. The required quantities of each of the vitamin standards are now being sent to national central laboratories for subsequent local distribution in countries abroad. The National Institute for Medical Research will continue to supply the international vitamin standards for use in Great Britain and Northern Ireland. The standards for the vitamins B_1 and D remain unchanged, and their supply at regular half-yearly intervals will be continued as before. The standard for vitamin A has been changed, a pure specimen of β-carotene having been adopted in place of the impure preparation of carotene hitherto employed. The unit of vitamin A remains unchanged, though it is now defined as the vitamin A activity contained in 0.6 microgram of pure β-carotene. In accordance with the recommendations of the conference, the β-carotene is issued in the form of a solution in oil, of which 1 gram contains 500 international units. The quantity of this standard solution supplied to each applicant is approximately 5 grams, and, on account of the small quantity available, it can be supplied only at yearly intervals, and not half-yearly as formerly. It is suggested, therefore, that care and economy should be exercised in its use, and that subsidiary laboratory standards exactly assayed in terms of the international standard should be prepared for routine work. *l*-ascorbic acid has been adopted as the international standard for vitamin C, the unit of activity being defined as the vitamin C activity contained in 0.05 mg. of pure *l*-ascorbic acid. A quantity of approximately 550 mg. will be supplied to all laboratories, institutions, and research workers in Great Britain and Northern Ireland requiring it. Laboratories, institutions, and research workers in Great Britain and Northern Ireland who do not receive the standards at present, and who desire to receive any or all of them, should apply to the Director of the Department of Biological Standards, National Institute for Medical Research, Hampstead, N.W.3.

PSYCHONEUROSIS AND INCAPACITY

We take this opportunity to correct a sentence in the leading article on " Psychoneurosis and Incapacity " in the *Journal* of March 16th reviewing Dr. James Halliday's inquiry into the incidence of psychoneurotic incapacity among insured persons. " Dr. Halliday found that no less than one-third of referred patients were, in fact, disabled by reason of psychoneurosis, though less than 2 per cent. of the certificates under which they were referred indicated that incapacity was probably due to conditions which might naturally be regarded as functional nervous disorders." In point of fact one-fifth of the patients considered to be disabled by psychoneurosis were certified to be suffering from this group of disorders. The figure 2 per cent. mentioned in our review refers to the incidence of psychoneurosis suggested by a scrutiny of the *official* morbidity statistics of the insured population as a whole. It is admitted that the 1,000 patients examined cannot be described as a random sample taken from the insured population since the circumstances of reference are not uniform ; but it would be interesting to have an explanation of the great discrepancy between the 20 per cent. certification of the psychoneurotic condition in the one group and the 2 per cent. suggestion of morbidity—due to this cause—in the other. Another point raised in our leading article was the allocation of persons with actual mental deficiency. There were only three such among the 1,000 cases examined, and these were allocated to the organic group.

An omission in the Birthday Honours list, published in our issue of June 9th, 1934, has just been brought to our notice. The Lord Mayor of Melbourne, Councillor Harold Gengoult Smith, upon whom was conferred the honour of knighthood, is a member of the medical profession, having the Scottish triple qualification and the diploma of M.R.C.P.Ed.

TREATMENT IN GENERAL PRACTICE

This article is one of a series on the management of some of the major medical disorders met with in general practice

THE TREATMENT OF WHOOPING-COUGH

BY

W. T. BENSON, M.D., F.R.C.P.Ed., D.P.H.

When an infant or young child who has not previously suffered from whooping-cough develops a persistent, troublesome cough with scanty clinical signs of bronchitis, and if within two or three days the cough, which is worse at night, tends to occur in paroxysms giving rise to suffusion or blueness of the face and occasionally terminates by a vomit, suspect infection with *H. pertussis*. If in such a case there is a history of recent intimate contact with whooping-cough then the clinical diagnosis can be made with some confidence during the catarrhal stage. Failing a history of exposure to infection the diagnosis may be aided by examination of the blood picture or confirmed by the isolation of the Bordet-Gengou bacillus.

The presence of a leucocytosis (commonly 15,000 to 25,000 white cells per c.mm.) with a well-marked relative lymphocytosis (from 50 to 70 per cent. of lymphocytes) is highly suggestive of whooping-cough. For bacteriological diagnosis two Petri dishes containing a special culture medium for *H. pertussis* will have to be obtained from a laboratory. Instruct the parent or nurse to expose the surface of this medium five inches in front of the patient's mouth for fifteen seconds during a natural paroxysm of coughing. Expose the second dish in a similar manner. Should the patient's cough be mainly short, sharp, and occasional, then expose each dish on four occasions. Return the dishes to the laboratory as soon as possible. Should examination of the cough plate after seventy-two hours' incubation show colonies of *H. pertussis* then a diagnosis of whooping-cough may be confidently made. The causal organism can apparently be isolated from approximately 75 per cent. of cases during the catarrhal stage of the disease. It is to be noted that a negative report does not exclude the possibility of whooping-cough.

It must be remembered that in infants and in adults the typical whoop may not appear throughout the whole course of the illness; bacteriological aid would simplify the diagnosis of such cases.

General Treatment

Most clinicians with an extensive experience of whooping-cough are of the opinion that when the primary essentials of medical care have been arranged for the patient we have practically exhausted our effective therapeutic armament. Vaccines, drugs, and ray therapy may be regarded as adjuvants to be given a trial when special indications arise.

When whooping-cough is suspected the patient should be confined to bed and strictly isolated from other members of the family who have not already suffered from the disease. Isolation should be enforced for a period of five weeks dating from the onset of symptoms. The bedroom, with a sunny exposure for choice, should be kept freely ventilated, but at an equable temperature around 60° to 65° F. If the weather is warm and sunny the bed may be placed at the widely open window during the day; otherwise the patient should be screened from draughts. The bedclothes should be light yet warm, and the child clothed in a flannel nightgown. A flannel binder firmly applied round the abdomen is of supportive value during the strain of coughing. Some trustworthy individual should be constantly in attendance to support and to comfort the child during the paroxysms of coughing and to have a receptacle ready for the vomit.

Providing the attack is afebrile, uncomplicated, and not unduly severe, the child may be allowed out of bed about the third day of the whoop. When the paroxysms exceed thirty in the twenty-four hours, or when there is a previous history of chest trouble, confinement to bed until convalescence is well established is advisable. If the weather is equable and sunny the patient should spend most of his time in the open air—precautions being taken to avoid contact with susceptible children. The clothing should be warm but not excessive, and mild exercise should be encouraged. The bowels should be regulated by occasional doses of syrup of figs or liquorice powder.

Importance of Diet

The dietetic management of a case of whooping-cough is of the utmost importance. In severe attacks, particularly if improperly handled, the persistent vomiting results in a serious degree of malnutrition. The secret of feeding is to give small amounts at frequent intervals. Large meals should be avoided, and food administered preferably about ten minutes after a paroxysm. For babies "feeding by the clock" may have to be adjusted to the necessities of the case. If vomiting and loss of weight continue then the interval between feeds should be decreased and the bulk of each feed lessened. Various modifications of the diet may be necessary. The milk may be diluted, boiled and citrated, or peptonized; whey and albumen water may have to be temporarily resorted to. In the worst cases mouth feeding may require to be supplemented by rectal or intravenous saline to which 5 to 10 per cent. of glucose has been added. Lavage of the stomach twice daily with 0.6 per cent. solution of sodium bicarbonate is a useful measure when vomiting is frequent and persistent.

With older children large meals must be avoided, but no material alteration in diet is necessary. Milk, eggs, butter, fish, chicken, meat, and vegetable soups are better than an excess of potato, bread, milk puddings, jams, and similar starchy or saccharine foods. Biscuits, rusks, oatcakes, or other dry crumbly foods should be avoided. Fresh orange juice drinks sweetened with 5 to 10 per cent. of glucose may be freely administered. A prolonged convalescence, with if possible a change of air, is advisable.

Vaccine and Serum Therapy

Very conflicting reports have been published regarding the value of vaccine treatment. It is generally agreed that no improvement can be expected to follow vaccine therapy unless the initial injections have been given during the catarrhal stage of the disease. A vaccine containing *H. pertussis* alone, or preferably a mixed vaccine contain-

ing in addition *H. influenzae* and the pneumococcus, may be employed. Six subcutaneous injections rising from 100 million to 1,000 million Bordet-Gengou bacilli should be administered on alternate days. This method of treatment has been given a prolonged trial in the wards of the Edinburgh City Hospital, but the results have proved disappointing.

Human convalescent whooping-cough serum or whole blood injected intramuscularly on one or more successive days in doses of 10 to 20 c.cm. has been reported on favourably by various French physicians. If circumstances permit this specific method of treatment might be tried in severe attacks in infants.

Drugs*

In spite of the numerous drugs that have been recommended for the treatment of whooping-cough the writer, after an extensive experience of many preparations—including belladonna, antipyrine, bromoform, benzyl benzoate, ether, adrenaline, and ephedrine—is of the opinion that none can be looked upon as a specific.

In a mild attack there is no need for medication. If desired a simple expectorant mixture may be administered thrice daily:

℞	Tinct. camph. co.	♏ iv
	Vin. ipecac.	♏ iij
	Syrup. scillae	♏ x
	Aq.	q.s.

When the paroxysms are frequent and severe, resulting in considerable exhaustion and loss of sleep, a sedative or antispasmodic drug should be prescribed. One or other of the following sedatives may be prescribed four-hourly: syrup of codeine phosphate, 10 minims; Dover's powder, 1/2 grain; or chloral hydrate 2 grains, with potassium bromide 2 grains. If necessary the dose may be cautiously increased until either improvement ensues or the child becomes drowsy. Phenobarbital (luminal) in doses of 1/6 grain to 1/4 grain three times daily occasionally results in a rapid amelioration of symptoms; because of its toxicity this drug should be employed with discretion. Belladonna is probably the most popular drug in the treatment of whooping-cough: in the dosage commonly prescribed it is quite ineffective. In an endeavour to control the paroxysms belladonna must be pushed to the physiological limit. Commence with three drops of the tincture three times daily; then, providing no idiosyncrasy is present, promptly change to four-hourly administration, followed by a daily increase of one drop per dose until dilatation of the pupils, flushing of the cheeks, and dryness of the mouth indicate that the limit of safety has been reached. During convalescence the patient should take cod-liver oil and malt with some preparation of iron; the syrup of the iodide of iron is excellent.

Ultra-Violet Ray and X-Ray Treatment

Whilst of value as a general tonic during convalescence, particularly in the winter months, ultra-violet light therapy has no specific effect on the paroxysmal stage of the disease. In the absence of pulmonary complications a daily general exposure of one minute to both back and front at a distance of thirty-six inches may be tried. The exposure may be increased by half a minute daily, care being taken to avoid pigmentation.

Although some American physicians have obtained beneficial results from *x*-ray treatment in 75 to 80 per cent. of cases, due apparently to a reduction in size of the bronchial lymph glands, other investigators have not been so successful.

* The doses recommended are suitable for administration to a child a year old.

Complications

The most important complications of whooping-cough are bronchopneumonia, convulsions, and gastro-enteritis. These complications are most apt to occur in young children, in whom they are a frequent cause of death. Bronchopneumonia usually develops at the height of the paroxysmal stage, and seems to predispose to convulsions. Gastro-enteritis is manifested by frequent vomiting and profuse diarrhoea in the intervals between the paroxysms. Subconjunctival haemorrhage, epistaxis, and bleeding from the mouth occasionally are seen but are not of serious import. Otitis media is not uncommon. Meningo-encephalitis and various forms of paralysis are interesting but very rare complications.

Prophylactic Measures

Whooping-cough is most highly infectious during the catarrhal or pre-paroxysmal stage. To control the spread of infection the patient must be isolated early in the catarrhal stage of the illness, and herein lies the importance of early diagnosis. Every precaution should be taken to prevent young children, in particular, from being needlessly exposed to infection with *H. pertussis*. Practically all the deaths from whooping-cough occur during the first three years of life.

Prophylactic vaccination is of value not so much in preventing the development of the disease as in modifying the severity of the subsequent attack. Five subcutaneous injections of 1,000, 2,000, 4,000, 8,000, and 8,000 million Bordet-Gengou bacilli should be given on alternate days to contacts who have not previously suffered from whooping-cough. The injection of serum or of whole blood of a whooping-cough convalescent or of those who have had whooping-cough at some time in their life has been reported as resulting in complete or partial protection in a large proportion of cases. For children under 3 years of age 5 c.cm. of serum or 20 c.cm. of whole blood should be injected intramuscularly.

In spite of the multiplicity of remedies recommended in the treatment of whooping-cough an effective therapeutic agent has yet to be found.

According to G. Bessau (*Med. Welt*, December 8th, 1934, p. 1715) tuberculosis in childhood almost always, but the later pulmonary tuberculosis almost never, follows a direct infection from without. Pulmonary tuberculosis, rare in childhood, never occurs before the age of 8, and its limited incidence increases as puberty is approached. In children lymph-gland tubercle almost always (and if there is softening or fistulization always) denotes a primary infection with primary focus, lymph channel, and regional gland affection—the triple primary syndrome of Ranke. Food infection gives tuberculosis of the mesenteric glands; inhalation, of the bronchial glands; infection of a carious tooth, of the submaxillary glands; infection of the tonsils, of the glands behind the angle of the jaw; infection of the nasopharynx, of the posterior cervical glands; infection of the external genitalia in girls, of the inguinal glands. Haematogenous infection of these glands in children is practically non-existent. Preventive measures consist in (1) prevention of infection, (2) prevention of extension from a primary lesion, which is never lethal *per se*. Human infection of the child is nearly always from a phthisical adult, of whom Bessau regards the "closed" as well as the "open" or sputum-positive cases as infectious: such infection should be prevented by isolation rather than by hygienic methods of disinfection or destruction of sputum. Isolation is all-important during the first half-year of life, after which the danger of spreading of the primary affect diminishes progressively. Bovine infection, Bessau thinks, is better prevented by pasteurization or sterilization of milk than by endeavours to produce tubercle-free milk from selected herds.

OSTEOPATHS BILL: SELECT COMMITTEE

SIXTH SITTING

The sixth sitting of the House of Lords Select Committee on the Registration and Regulation of Osteopaths Bill was held on March 22nd, Lord Amulree presiding.

Dr. W. Kelman Macdonald of Edinburgh, before leaving the witness-chair, was asked some further questions by Lord Dawson of Penn, a member of the committee. The witness agreed that, with regard to pernicious anaemia, liver treatment had now been so refined that patients receiving an injection once a week could pursue their ordinary work. Lord Dawson took up the case of epilepsy which the witness had quoted, and asked whether there was any objective evidence for his contention that a displacement of the cervical or dorsal vertebrae was a factor in causation. He suggested three lines of possible evidence: *x*-ray pictures of the abnormalities, treatment of a series of cases of epilepsy by manipulations only, and the findings at post-mortem examination. Dr. Macdonald replied that nothing had been done post mortem; very little such evidence would be available post mortem unless the lesion was of very long standing, six years or over. When the ligaments of a joint were cracked the vital characteristics of lesions disappeared, and all that were left were bony changes provided the lesion had been in existence long enough for these to appear.

Lord Dawson: Why I chose epilepsy was because it is a concrete example which will appeal, and if something more could be done for it than at present it would be a great public service. What about *x*-ray investigation? You must have a wealth of material accumulated somewhere which will show these deviations. Can we have it in a systematic way as matter is brought forward in our medical societies?—In epilepsy I have no such evidence that I can produce. All I can state is what clinical results we get. With regard to *x* rays it is rare that the stereoscopic plate fails to show a lesion which is palpable to the fingers, though it is true that small maladjustments are sometimes not shown on the plate when considered to be present by the evidence of the palpating fingers.

There is surely clinical evidence of alleged displacement in epilepsy?—We cannot bring up here evidence of every disease and condition.

Is it available anywhere?—In the *Journal of the American Osteopathic Association*.

Perhaps we might have that evidence produced.—I will be glad to produce it. I do not know that there is anything on epilepsy itself.

Is it not laid out as I would lay it out if I were going to a medical meeting, with *x*-ray photographs?—I cannot produce that.

Lord Dawson said that he was anxious to clarify the position. He was trying his best as a member of the tribunal to be impartial, but he was also a member of the medical profession. He would never hesitate to ask an osteopath to help him over a case if he thought it was for the patient's benefit. One statement made at the last sitting had been misinterpreted in the Press, where it was suggested that it was a serious thing that a medical man sending a patient to hospital should not be allowed to go there and consult over the case. But he was not aware of any hospital where, if he sent a patient into the public ward, he would be allowed to "butt in." Imagine the confusion that would ensue if he interfered with the responsibility of those in charge. Dr. Macdonald agreed. Mr. Thorpe, K.C., for the osteopaths, said that in Lord Dawson's case the patient in hospital would continue to be treated from the medical standpoint, but the ground of complaint in the case instanced was that if a patient had been treated osteopathically that kind of treatment would cease on his entrance into hospital, although he desired it continued.

Osteopathic Claims

Lord Dawson then asked the witness if osteopathy as a system of healing covered all the diseases of the human body, to which Dr. Macdonald replied that they claimed to be complete physicians.

That being so, you include in your embrace diseases which carry a definite mortality. If you are going to take responsibility for all diseases, acute or chronic, surgical or medical, would you not claim to sign death certificates? I should regard it as not consistent with my self-respect if, after attending a patient for weeks, and struggling for his life, I had eventually to submit myself to a coroner.—I can imagine the righteous indignation you would feel, and we feel it just as righteously, but we must have a beginning somewhere. We cannot go to Parliament and say we want all these privileges until we have proved that we have educational colleges where students can get the necessary education for these privileges. Therefore, being anxious to have some preliminary recognition, we are willing to give way on many points.

What is to prevent your signing death certificates if you are fit to treat people?—Our proposed curriculum was at first one of four years, and I for one could not support the claim for the right to sign death certificates on such a curriculum.

The witness added, in reply to a further question, that if a five-years curriculum were established, osteopaths should certainly sign death certificates; but the signing of death certificates was an important privilege, and the certificate of a medical man himself was not always accepted.

Mr. Thorpe: I am chiefly responsible for the fact that this clause (privilege to sign death certificates) is not being pressed for, but if my Lord Dawson thinks it possible I shall be glad to put the clause in.

Lord Dawson next put certain questions to the witness on the divergencies between osteopathic and medical treatment of disease. It had been advanced in evidence that the human body contained within itself all substances required for the treatment of disease. Dr. Macdonald said that he could not maintain that it held substances; what he had always stated was that it contained the power necessary for the purpose. Lord Dawson said that he had extracted from Dr. Macdonald's and Mr. Streeter's evidence admissions with regard to over twenty diseases—acute diseases, parasitic diseases, metabolic diseases—that drugs were necessary. That meant an assimilation of osteopathic practice to the practice of medicine. Dr. Macdonald replied that they (osteopaths) never used drugs in acute disease, and they were of opinion, rightly or wrongly, that their treatment was superior to drug treatment.

Lord Dawson: Take serum for diphtheria. I was impressed by your downrightness in saying that you would use serum.—You will also remember that I gave a list of conditions which I called medical emergencies. I pointed out that these emergencies were met medically by certain methods, and with the exception of intravenous injection of saline and the use of a heart stimulant in non-toxic heart failure, all these were dealt with by products of the ductless glands. That takes away a great number of the exceptions.

This answer caused Lord Dawson to exclaim that he began to despair of this inquiry. "I cannot nail a single thing to the counter. I cannot get one single fact home. Unless we can get answers to certain questions this inquiry is futile. I have never been mixed up in an inquiry where it has been so difficult to get an answer."

What is a Drug?

Dr. Macdonald agreed that in diphtheria he would use anti-diphtheritic serum; in syphilis he would give arsenic or mercury; in meningococcal meningitis he would use serum, though he pointed out how rare this condition was compared with other forms of meningitis for which no serum was available. Lord Esher said that there was evidently a difference between Lord Dawson and the witness in regard to what they meant by drugs.

Lord Dawson: You [osteopaths] resemble homoeopaths in this respect, that you have methods of treatment profoundly divergent from the methods adopted in orthodox medicine, but homoeopaths are content to go through the medical curriculum and to teach their special tenets after qualification. The osteopath says there are too many drugs; the homoeopath says he cannot get drugs enough. Homoeopaths run a hospital and so forth, and co-operate increasingly with the orthodox profession.—I quite agree, but I should like to point out that the difference between the homoeopaths and allopaths is entirely one of the dosage in drug treatment and the variety of drugs; there are not the basic differences that exist between osteopaths and practitioners of orthodox medicine. With regard to the causation of disease homoeopathy is the same as allopathy.

A little later Dr. Macdonald said that homoeopaths were inside the profession, and had "killed" themselves as a school of thought, in his opinion.

Lord Dawson said that as regards treatment there was a big divergence between orthodox medicine and homoeopathy, as wide as between orthodox medicine and osteopathy. It was as difficult for him to appreciate homoeopathic dicta as osteopathic. Turning to the cause of disease, he characterized as the most startling and revolutionary statement the osteopathic claim that the osteopathic lesion was the most important single factor. It was this claim which led to the proposed setting up in this country of another system of medicine with separate curriculum, examiners, and registration. It was the first time such a claim had been made. To set up such a system parallel with that of the older medicine would result in a great amount of administrative inconvenience. It would mean duplication of the medical units in the fighting services and in the services of which the Ministry of Health was the head. It would be like having two classes of barristers, one of which went through the Inns of Court and the other did not. "That definition given by osteopathy as to the causation of disease has to be proved up to the hilt before it is justifiable to proceed. We must have the recorded tests of scientific bodies, not odd collections of personal experience."

"What more, my lord, can we do?" said Dr. Macdonald in reply. "I have worked for twenty years with this self-same spinal lesion as the only factor in ill-health to which I paid any attention so far as treatment is concerned. As a result of that twenty years' work I am satisfied that the medical profession has missed a great truth. I have produced to the Council of the British Medical Association and yourself the nature of the research work available—the seven bulletins [of the A. T. Still Institute] on osteopathic research. I am willing to go over this research work with the committee. Up to this present time I have not been asked a single question on the scientific basis of osteopathy. I contend that our opponents have no right to make the statement that there is no possibility of scientific investigation. I give them the lie direct."

Dr. Macdonald later explained that he was referring to the statement put in last week by Sir William Jowitt signed by some hundreds of members of the medical profession.

The Question of Scientific Proof

Lord Dawson: It is a rule in scientific work that a person who puts forward a new theory has to prove it. It is for the man who puts forward the theory to "deliver the goods" before his colleagues.

Dr. Macdonald: We are here pushed in front of your lordships by public opinion following the results of our work on sick people. We have what we consider to be proof that our contentions are at least worthy of scientific investigation, and we think, further, that the osteopathic case has been proved on a scientific basis adequately enough for absolute acceptance even by a hypercritical medical profession. Well, perhaps not that, but at any rate more than enough to justify our presence here. We welcome any tribunal that will go into the scientific aspects of it, but we deny their right to make such a statement as I have mentioned. Would you put your name to such a preamble?

Lord Dawson said that he himself had shown no bias at all. The witness had agreed that the single principle on which the case stood or fell was on this question of causation of disease, and as the proposals in the Bill were of a revolutionary character involving large changes from the administrative point of view, it was necessary, as he had said, that the case should be proved up to the hilt. What were wanted were "marshalled facts." Dr. Macdonald agreed; he submitted definitely that there was scientific evidence for osteopathy, but added that this was the first time that any responsible official body in this country had ever announced its willingness to investigate osteopathic claims.

Lord Dawson: When I want to look into the work of any profession I go to the library and take down their published proceedings. Where can I get similar proceedings in regard to osteopathy?—All our research work is contained in the seven bulletins of the A. T. Still Research Institute and two or three dozen articles in the *Journal of the American Osteopathic Association*. I will make it my business to have all these articles, not summarized, but brought together in a convenient form before your lordships, and you will then have the case before you.

Lord Marley, taking up Lord Dawson's point about duplication of administrative services, pointed out that in the chaplains' department of the fighting services there was an arrangement whereby chaplains of different denominations could work side by side. It should be quite possible to provide two kinds of medical treatment. Lord Dawson asked about hospitals. Lord Marley cited the instance of the Los Angeles county general hospital, which had medical and osteopathic units without any difficulty arising.

Lord Esher asked whether the medical profession had really been watching osteopathic treatment for twenty years and never investigated the question for themselves.

Dr. Macdonald: "I assure you they have not. If they have I challenge them to say."

These disinterested scientists have been watching this for twenty years and have never taken the trouble to investigate it?—That is my assertion.

It is not a question of etiquette?—That is not for me to say.

The Chairman asked for a definition of a drug from the point of view of the osteopath. Dr. Macdonald hesitated, and then said:

"You would think such a simple question could be simply answered. A drug is a chemical which is given for therapeutic purposes in order to alter the quality of the body reactions. That is the kind of drug we do not wish to administer. Osteopathy removes the possibility of interference with the normal reactions of the body."

Lord Marley: In point of fact, if you got these cases at an earlier stage you would be able so to improve and build up their lines of communication that they would never reach the stage where drug-giving was necessary?—Most essentially so.

You admit that there are external accidental injuries which could certainly cause disease in an otherwise osteopathically proof body?—Yes.

Lord Dawson: Are there not among the twenty conditions which I mentioned earlier a number of acute invasions of which we get no warning? Typhoid sometimes comes on very acutely.—The witness was understood to reply that if typhoid fever had developed one could only deal with the patient as one saw him.

No time to parley in the gate?—I agree.

And if it is bacillary dysentery we have to get serum in as quickly as possible or we lose the patient. Diabetes sometimes comes on very suddenly. (Lord Dawson gave an account of a patient who developed acute diabetes within twenty-four hours.) We should not stop to talk about osteopathic treatment; we should get down to it?—That would be one of my "medical emergency" admissions.

Lord Marley: Would not these cases quoted by Lord Dawson be, in fact, effects, the probability being that those who did develop amoebic dysentery were in some way osteopathically unsound?—Yes.

Educational Facilities

In reply to the Chairman, Dr. Macdonald said that when he went to America in 1910 he found more students at Kirksville than in the medical school at Edinburgh—there were 600 or so. He gave particulars of the professorships and of the hospital of 200 or 300 beds attached. The idea here would be, in the first place, to arrange for the preliminary sciences, and anatomy, and even pathology, to be taught in some medical school. No student would be allowed in college until he had passed the preliminary medical matriculation examination. He should spend the first two years in the medical school, and after that take only pathology there, with clinical experience in the hospitals, osteopaths having no such institutions. In the first year there might be ten or twenty undergraduates, and a very much larger number of post-graduates. This last week he had had three Edinburgh graduates who wanted to study osteopathy. "The number would increase tremendously if we got recognition." In the projected college there would be about eight lecturers and up to a dozen or more clinical instructors, but the students would have no access to osteopathic hospital beds, only to clinics. He thought that people who came from America to practise over here ought to do an extra year in the college. In the British Osteopathic Association, which was one of the bodies nominated in the Bill as appointing a member to the board, about half the members were of American birth, he believed, and there were some who had not been naturalized in this

country. He believed that all the members of the Incorporated Association of Osteopathy were graduates of Dr. Littlejohn's School. The Osteopathic Defence League was a lay body, consisting of people anxious to further the cause of osteopathy.

On leaving the witness-chair, which he had occupied for twelve hours, Dr. Macdonald thanked the tribunal, and especially Lord Dawson of Penn, for their "willingness to understand us in our heresies."

More Grateful Patients

At this point the evidence of three patients was interposed. Mr. Horace B. Bennett, *M.C.*, gave evidence as to the treatment of himself (for liver disorder), his wife, and his sister successfully by osteopaths. Mrs. Florence MacNaught stated that, several years ago, she had been diagnosed by the family doctor as suffering from exophthalmic goitre, and was told by three specialists in London that her condition was incurable. She then saw an osteopath, who stated that the whole trouble was that "the bones at the back of the neck were sticking out," and treated her for about three or four months, after which the complaint subsided. She afterwards married, and went out to a strenuous life in the jungle of Siam with her husband, and had had no return of the trouble. Mr. Walter William Hughes, a station foreman, gave a history of injury to the leg followed by rheumatism of such a nature that the doctors told him he would be crippled for life. He had electrical treatment, and was worse; then had osteopathic treatment, and was a new man.

None of these witnesses were cross-examined.

The Evidence of Dr. J. M. Littlejohn

The next witness was Dr. John Martin Littlejohn, head of the British School of Osteopathy, who, in answer to Mr. Murphy, for the applicants, stated that he held the degree of D.M. of the Dunham and Hering Medical Colleges in Chicago, and D.O. of the Kirksville School of Osteopathy. He was educated at Glasgow University, and took bachelorships in law and divinity and a medal in forensic medicine. He took up the study and practice of osteopathy in the United States, where he resided for almost twenty years, and taught osteopathy for nearly sixteen years, becoming professor of physiology and dean of the faculty at Kirksville in 1898–9, and professor of physiology at the National Medical School, Chicago. He was president of the British Osteopathic Association in 1925. He established the British School of Osteopathy in 1917. The school was supported entirely by those who sought to take advantage of its teaching; there was no endowment, and there were no voluntary contributions. No one on the staff was paid except two clinic superintendents. He, himself, had his own private practice in Dover Street, London. The number of students in the school at the present time was forty-five, and since the school was founded ninety-eight diplomas had been granted. "We tell our graduates they have no right to call themselves doctors of osteopathy; the title we give them is 'diplomate in osteopathy,' and we say that they should add 'B.S.O.Lond.' to specify the school from which they come" He added that medical men attending the school were required to undertake two years' course (instead of four). Two medical men had been at the school, one for a year and another for nine months, but did not complete the course. On request he wrote the names on paper and handed them to the Chairman, Mr. Dickens asked if he could see the names, but the Chairman said that the names would not be divulged.

Dr. Littlejohn went on to say that the students at the school were required to have attended some school recognized by the medical profession for pre-medical work. Third and fourth year students were required to treat individually under supervision 700 patients in the attached clinic. Nine demonstrations were held every week in which patients were examined in front of the students, and a careful diagnosis was made of the conditions found. There were thirteen affiliated clinics in and around London and in the country. The school was situated at Buckingham Gate in substantial and well-equipped buildings.

He had no objection to the extension of the course to five years. The pre-medical work and anatomy and physiology, plus dissection, required at least two and a half years, and the course in osteopathic work in its outline required a good three years. The Bill would bring many advantages. At present it was difficult to convince intending students that there was a proper field for osteopathic practice. It was anticipated, if the Bill passed, that use would be made of the medical schools for the part-education of the students. At present no provision could be made for the students to see hospital cases. When the Bill passed it was hoped that osteopaths would have their own hospital or that there would be hospital access for students whereby they would obtain experience in acute diseases. The witness gave some particulars about osteopathic education in America.

Cross-examination by Sir W. Jowitt

In cross-examination by Sir William Jowitt for the British Medical Association, Dr. Littlejohn was asked about his degrees. He said that when he got his "full medical degree" Chicago University was in formation; there were a number of medical schools which had since been incorporated. Dunham was one such college; it had at that time about eighty students. He had forgotten the names of the teaching staff. For hospital experience the students went to Cook County Hospital, which belonged to another college. He had forgotten the names of the doctors and surgeons there, but he remembered Dr. William Osler (as he then was) coming at times to demonstrate. It took him four years altogether to get his medical degree. Asked about his other degrees, he said he was LL.D. of Texas. This was an honorary degree. Asked for what it was conferred, he replied, "public work—in political philosophy, for example," and added that he was a fellow in political philosophy in Columbia University for two years. He applied to become a fellow, presenting for the purpose a thesis on "The Political Theory of the Schoolmen and Grotius." With regard to the honorary degree conferred by Texas, he said that he "did not know why or how or anything in reference to it, beyond the fact that the honorary degree was conferred upon me." He began his teaching career as a tutor in mathematics and physics. He was one of the foundation scholars in natural philosophy in the University of Glasgow under Lord Kelvin; at that University, where he was, with a year's intermission, from 1881 to 1892, he took the degrees of M.A., B.D., and LL.B. He went to the United States in 1889 and again in 1892.

American Degrees

Did you meet while in America a gentleman called Mr. Looker?—I never met him.

Did you meet him over here?—Yes.

He had to leave America rather quickly?—So they said.

And Mr. Looker came to this country and established a school for teaching osteopathy and chiropractic?—I don't know.

Was it not called the Looker College of Osteopathy and Chiropractic?—I only know it was called the Looker College of Osteopathy.

It used to give people "M.D.," which means "master diagnostician," and "F.B.C.S.," which means "Fellow of the British Chiropractors Society?"—I do not know anything about that.

What I am suggesting to you is that Mr. Looker, who had left America rather hurriedly, finally left England rather quickly?—He died!

Before Mr. Looker left England you took over the students?—Some of his students, yes.

And you arranged to give his students the degree of "D.O."—We do not give any degrees.

The diploma of "D.O."?—We arranged to give them the diploma of "D.O." on the condition that, first of all, they should give their credits from the Looker School, and, secondly, that they should fulfil a certain amount of work in connexion with the school.

Did you do this: Did you send the Looker students round a paper of questions? Did you let the students take the papers home with them, and then in about a fortnight's time did they send in the answers?—That was tutorial instruction, not examination.

If that was examination I strongly suspect most of the students passed?—That was not examination at all. They were supplied with a series of papers over thirteen or fourteen months.

They were test papers to see how they were getting on?—They were test papers, and along with each test paper there was sent a specific course of study with books which they were required to read.

Sir William Jowitt read what he said was one such question: "State exactly what you would expect to find in a patient on which you would base a diagnosis of (*a*) asthma, (*b*) colitis, and (*c*) pneumonia." Sir William added, "I myself could go home and get a medical dictionary and make a good job of it." He also showed the witness a photographic copy of a letter, written from the British School of Osteopathy, November 14th, 1928. This was addressed to someone described as "Dr.," who had been at the Looker School—"'Doctor,' I suppose, because he called himself doctor?"; to which Dr. Littlejohn retorted "By courtesy, the same as many medical doctors." The letter, which Sir William Jowitt read, contained the passage: ". . . We are allowed to accredit graduates of an unrecognized school to come in on three years' credit and to require one year's work." The letter went on to mention a course covering one year, giving eight test papers to cover the work, "and a final formal examination, so that we could say we had conformed to every condition."

You let a man come from an unrecognized college and credit him with three years' work on the strength of eight test papers?—Crediting him with three years' work.

Without knowing what he has done if he has been to an unrecognized college?—Provided he proves it.

And he proves it by eight test papers? What do you mean by the phrase in the letter, "so that we could say that we had conformed to every condition"? Does that mean that the diplomas you were giving were merely colourable?—No.

Sir William Jowitt asked the witness to bring at the next sitting his American degrees for inspection, and Dr. Littlejohn promised to do so. Counsel exhibited an American degree conferred on the "doctor" to whom the letter just read had been addressed, and reading, "We hereby confer upon you the degree of Doctor of Medicine and Master Diagnostician," signed by a lot of people. The recipient apparently got that as a result of his studies at the Looker School.

What would you think of the conduct of a man who came over here and described himself as "M.D." if in fact he meant "master diagnostician"? Would not you think he was a fraudulent rogue?—I would.

And "F.B.C.S.," meaning "Fellow of the British Chiropractors Society"?—I should interpret it in that way.

"And so should I," commented Sir William Jowitt.

The cross-examination had not concluded when the committee adjourned.

SEVENTH SITTING

Dr. Littlejohn's Cross-examination Continued

When the Select Committee resumed on Monday, March 25th, Sir William Jowitt continued his cross-examination of Dr. J. M. Littlejohn.

Dr. Littlejohn admitted that it was a mistake when he had stated at the previous hearing that he held a degree of the University of Texas; the degree was that of the Add-ran Christian University in Texas, a college founded by a religious body called the Disciples. He handed in the thesis which he had submitted for the Doctorate of Philosophy of Columbia, but said that owing to illness he never proceeded, and in fact was not a doctor of philosophy. Sir William Jowitt drew attention to certain documents, one of them issued by the British School of Osteopathy, in which its principal was described, along with other distinctions, as "Ph.D., 1894." Dr. Littlejohn said that this was a mistake, and had escaped his notice.

In further cross-examination he said that he had been professor of physiology, Dunham (a school no longer existing, in Chicago), and professor of applied physiology in the National Medical University, Chicago, 1904–6. Asked what physiology was, he replied that it was "the applied anatomy of functioning in the human body." Answering further questions with regard to his career, he said that although he had had no osteopathic training until 1897 he became in the following year dean and professor of the American School of Osteopathy, but he had had previous training at Glasgow University in chemistry and elementary physiology, and at Anderson Medical College in anatomy and physiology. He went to America in 1889, partly for health reasons, but also to carry out research.

The British School of Osteopathy

Dr. Littlejohn was next asked about the British School of Osteopathy. The number of students in the school to-day was forty-five, and during last year eleven diplomas were given. He believed nine or ten of the forty-five were attending, or had attended, medical schools. The requirement since the autumn of 1934 had been that every student must present the pre-medical certificate. The students had to take matriculation, and, after that, two courses at a medical school. They were also required to put in a certain number of hours at the osteopathic school during the first and second years, but as there had been difficulty in obtaining their admission to medical schools in London, and some of them attended provincial medical schools, they were allowed to make up during their third and fourth years at the school for the loss of time occasioned by their absence from London during their first and second years. In ordinary circumstances, however, a student was expected to attend at the osteopathic school concurrently with the medical school. He stated that last year forty applicants were rejected because they could not, or would not, go to medical schools, and eight were accepted who were attending some medical school. Certificates were issued only to those taking the full course. Sir William Jowitt put to the witness the case of a Mr. George Spencer, one of the Looker students, who came to his (Dr. Littlejohn's) school for one year, and then received the following certificate:

> "Be it known that George Spencer, having completed the required studies, and attended the full course of lectures, clinics, and demonstrations during the regular four-year course and having passed satisfactory examinations in all branches of training according to this School, including clinicals and other practical demonstrations, is hereby admitted to the diploma in osteopathy. Given at London, 22 December, 1928. J. Martin Littlejohn, Dean of the School and Superintendent of Education."

Asked whether that was an honest document, he said that it was; they were taking a student who gave evidence that he had completed three years' work in an irregular school, and they required him to take one year's work and pass an examination. His school had no relation with the Looker school, but in 1925 a certain number of graduates of the latter, having formed an association called the Incorporated Association of Osteopaths, Ltd., made application for admission to his school, and got credit for the work they had done in the irregular school.

Is it honest to give a certificate that a man has attended four years at your school when in fact he has not?—He has completed the four years according to our requirements.

Do you not suggest that he has completed a four-years course at your school?—No. (Several members of the Committee: It says so.) It means he passed an examination; it does not mean he was taught at the school.

You are stating that the man during four years attended the regular course of the school. Is that an honest document?—Yes.

Lord Elibank: Was that document drawn up by a lawyer?—By our solicitors.

Sir W. Jowitt: No English solicitor, I suggest to you, could possibly have passed that document?—I hold that it is quite in conformity with our school.

The Looker students could get their diplomas not only in osteopathy but also in chiropractic as a result of a six-months course?—In chiropractic, yes.

And in osteopathy?—I don't know. I know nothing about the school.

Is it not a fact that a student might get his diploma in osteopathy and chiropractic from the Looker school on the strength of a six-months course, and a correspondence course to boot?—I cannot answer that question.

A little later Sir William Jowitt said: "I challenge the statement that your existing students are attending the medical schools. Will you write down their names on paper and hand it to the Chairman?" The witness said he would have no objection. Two of his present students, he said, were fully qualified medical men, holding the conjoint diploma.

The Equipment of an Osteopathic School

Taking up a reference to "ten-finger apparati" in the prospectus of the school, Sir William Jowitt asked what other "apparati" were available. The school was announced to teach "x radiance," ultra-violet therapy, and so forth. Witness replied that "apparati" were not required for these purposes. It was only necessary to teach the students to know when to send patients for these lines of treatment. The school had no *x*-ray apparatus, but students were taught to read and interpret the *x*-ray picture. There was a small laboratory for testing blood and urine, and there were "scores of microscopes."

Questions were next asked about the staff of the school, none of whom were paid except the secretary and the witness's son, who was clinic superintendent; the dean received nothing. The school had the services of a specialist in the ear, nose, and throat. The witness himself took the instruction in the eye and the skin. There was also a teacher in genito-urinary diseases, in osteopathic orthopaedics, and in obstetrics and gynaecology, while he, himself, gave a few lectures in medical jurisprudence.

Finally, Sir William Jowitt put to the witness a letter which had been received by himself and by members of the committee from a Mr. A. D. Wareing, formerly secretary of the school, with regard to the bodies proposed under the Bill to have the right of appointment to the Board, pointing out that the British Osteopathic Association was largely American in character, and that the British School of Osteopathy and the Incorporated Association of Osteopaths, which were each seeking to elect one member, were related in that the latter body was composed of graduates of the former. Dr. Littlejohn thought the same duplication might be shown in the composition of the General Medical Council. He admitted that perhaps fifteen of the sixty-seven members of the Incorporated Association were Looker students.

There are a large number of osteopaths in this country who are not members of any of these bodies. Don't you think they have just as good qualifications—or just as bad—as the students of the Looker school?—That would be for the board of examiners to decide.

In reply to Mr. Radcliffe, K.C., for the Royal Colleges, the witness said that of the ninety-eight graduates from his school, twenty-six had had some medical training. He only knew of two of them, however, who had passed the medical examination in anatomy. Many of the others were taught descriptive anatomy at the Chelsea Polytechnic as well as at his own school, but not with the desired facilities.

Is it not perfectly plain that the British Osteopathic Association will not recognize your diploma?—It may be.

Do you not know it is so?—Not officially.

You claim to teach your students the treatment of acute diseases?—We teach them the theory, but not the practice; we have no facilities. We have money promised for the building and equipment of a good hospital.

It will be for the treatment of acute diseases?—I hope so.

Although no osteopathic doctor is to have the power to sign death certificates?—Yes.

Will that be a satisfactory hospital?—It will not be satisfactory.

A remark by the witness that his school had 10,000 microscope slides of pathological conditions led Lord Dawson to ask whether any of these showed the osteopathic lesion, but he said that the slides were imported, none of them were made in connexion with their osteopathic work.

Asked how he could deal with such conditions as diabetes or pernicious anaemia without laboratory facilities, witness said that the condition was discovered and the patient recommended to go to a specialist.

Do you mean a physician?—Yes.

You are pushing your cases on to the medical profession. Then you are not fit to cover the whole ambit of disease?—We are not doing so at the present moment.

Witness promised to let Lord Dawson have the clinical sheets of the 18,000 patients of the clinic. In reply to further questions as to radiological and other evidence for the osteopathic concept, he said that the evidence was not marshalled.

Lord Elibank asked the witness what had led him to improve his curriculum in 1934, and he replied that it was due to the hope that osteopaths would get some recognition. Re-examined as to his degrees, he agreed that there was no doubt that he was a Fellow of Columbia College, afterwards the University, and that as to Dunham, of Chicago, of which he held the degree of D.M., this had been incorporated with Hering College in the same city. With regard to the British School of Osteopathy, he himself had never made a penny out of it.

This concluded the case of the applicants for the Bill.

THE CASE AGAINST THE BILL

The case for those opposing the Bill was then opened. In the temporary absence of Sir William Jowitt, leading for the British Medical Association, Mr. H. F. Dickens said that the Association, as representing the general body of practitioners in this country, was prepared with evidence, chiefly that of Sir Henry Brackenbury, which was intended to impress upon the committee certain practical considerations arising out of the Bill from a social, political, legal, and administrative standpoint, and this without any regard whatever to the question of the truth or otherwise of the theory which the other side had put forward. The witnesses for the British Medical Association, when they gave their evidence, would be guided simply by considerations of the State and the good of the public, and not by any prejudices which some persons might think attached to them as members of the medical profession. But in the meantime, as the case had inevitably drifted to a certain extent into the consideration of the value of what had been called the osteopathic concept, as to which their lordships had already heard a great deal of evidence on one side, it might be well that evidence should first be tendered on the other. A statement, signed by some eight hundred men and women engaged in teaching biological and medical sciences, had already been put in against the Bill, and the signatories had appointed five representatives to give evidence—namely, Sir Farquhar Buzzard, Regius Professor of Medicine, Oxford, Professor G. E. Gask and Professor E. H. Kettle of the University of London, and Professor H. A. Harris and Professor Joseph Barcroft of the University of Cambridge. These witnesses were not represented by counsel, but Mr. Dickens suggested that in view of the way the proceedings in the committee had developed their evidence should be taken first.

Lord Dawson said that the arrangement was inconvenient in this respect, that Dr. Macdonald, the chief witness for the osteopaths, had promised to submit within a week the body of written evidence on which the osteopathic concept was founded, and it would have been very useful to have had these witnesses at a later stage when that evidence was available so that questions might have been put to them upon it.

Memorial by Professors and Lecturers in Biological and Medical Science

Sir Farquhar Buzzard, Bt., then read the following statement to which some 800 signatures were appended. He said that a great many of those who had signed the statement were not engaged in medical practice and a number were without medical qualifications. Among them were twenty-three Fellows of the Royal Society, forty Doctors of Philosophy, seventy-five Doctors of Science, and 100 Masters and Bachelors of Science. The passage of the Bill into law would not affect any of them personally, and their objections to it were based on its menace to the interests of the public and to the prestige of scientific medicine in this country in the eyes of the world.

The statement was as follows:

1. The signatories to this statement are engaged, as university professors and lecturers, in research and in teaching in the fields of biological and medical science. Many of them are not engaged in medical practice and a number are without medical qualifications. The tenure of their posts

commits them to no particular scientific theories or conceptions: on the contrary they enjoy the intellectual freedom that is characteristic of British universities.

2. The practice of medicine and surgery is based on our knowledge of the anatomy, physiology and pathology of the human body which is the product of careful and often brilliant research in the past, which is always growing and which involves, for its understanding, an intimate acquaintance with the elementary principles of such other sciences as biology, chemistry and physics. For this reason the medical student, in the period of the six, seven or more years which precedes his qualification to practise, is the pupil of teachers in all these subjects. In other words the medical curriculum is designed not only to equip a student for his vocation but to give him a scientific education such as will enable him to keep pace with the constant progress of knowledge and with the changes of practice in the profession of his adoption. More than that, his training should make him an intelligent critic of new theories, and competent, with the safety of the public at heart, to adopt or reject them according to his judgement.

3. In the laboratories of the universities, workers are constantly engaged in the examination of current doctrines and theories and in the testing of new propositions. No hypothesis which is supported by evidence capable of scientific investigation is rejected without careful and disinterested exploration and experiment by men and women adequately equipped by knowledge and experience. There is nothing to prevent—indeed, there is all to encourage—the recognition and acceptance of a doctrine or theory based on truth. In short, the aim of scientific workers is the discovery of truth, not the advancement of a particular doctrine or notion.

4. The theory of "osteopathy" with its conception of the "osteopathic lesion" as the primary cause of disease is unsupported by scientific evidence. No biologist, anatomist, physiologist or pathologist of a British university has observed this "osteopathic lesion" and no evidence capable of verification has been brought forward. If this "lesion" exists, it is unbelievable that it has escaped the observation of the workers in these sciences and that evidence of scientific value cannot be brought forward in support.

5. Biological and medical sciences progress steadily as the result of research and discoveries, and the teaching of our universities is changed or amplified from time to time in the light of increased knowledge. The fundamental conceptions of these sciences are based on carefully controlled observations which have stood the test of time—and it is these fundamentals that osteopathy controverts. If the osteopathic theory, based on the idea that a single cause, or "lesion," is responsible for all or most disease, is true, the university authorities should, logically, supplant the teachers in their scientific and medical faculties by disciples of the osteopathic faith. There can be no compromise; the theories of medical science and of osteopathy are incompatible and contradictory.

6. It is clear from the above statements that official recognition of osteopathy as a theory of medicine completely opposed to the teaching in British universities, and indeed of universities throughout the civilized world, would create far-reaching and revolutionary changes in scientific medical and biological education.

7. The signatories respectfully request that the following may give evidence in support of the above statements: [Names given above].

Sir Farquhar Buzzard's Evidence

Sir Farquhar Buzzard next tendered evidence on his own behalf. Dealing first with the definition of osteopathy given in the Bill, he said that if by "structural integrity" was meant the integrity of every atom of the body mechanism, the definition was a platitude; if "structure" referred chiefly to bones and joints the emphasis on structural integrity as the most important single factor was merely a matter of opinion. Many unqualified practitioners, for instance, would regard the administration of herbs as the most important factor in the maintenance of well-being. There was probably a difference of opinion among doctors on the subject. Some might plead for a diet rich in vitamins as the most important single factor, others for a daily evacuation of the bowels; no one would put drugs in that position. The Bill was designed to establish a system of the healing art the basic principle of which must be accepted without question by its students before they had any scientific or medical training. No medical student was asked to do this. He was put through a course of training far more thorough than that contemplated in the Bill, but was free to form his own opinion as to the most important single factor, if he cared to make such a generalization, and to base his practice thereon. If he accepted the osteopathic idea as the right one he was in a position to treat his patients by manipulative methods. It was worthy of consideration that thousands of students had received a medical education in which there was no attempt to implant any particular doctrine, and only a minute fraction of them finally adopted osteopathy as their guiding principle.

From time immemorial there had been cults with various names, each proclaiming its newly discovered single factor. If anyone had travelled in America, as he had done of recent years, he would be convinced that the doctrine of osteopathy was dying in the country of its birth. Here Sir Farquhar quoted from a book published six years ago by Dr. H. W. Haggard, professor of physiology at Yale, devoted to the story of the science of healing:

> "Chiropractic treatment is an old form of faith cure under a new name; its forerunner was osteopathy, and the forerunner of osteopathy was the bone manipulation practised by 'joint adjusters' or 'bone-setters' . . ."

The author went on to say that 80 or 90 per cent. of all patients got well under any treatment, or none, but when one of the patients of these cults got well it was invariably attributed to the virtues of the treatment. Any cult at any age could produce a number of grateful patients, and even every one of the 55,000 medical practitioners in this country could produce as many as Dr. Macdonald had thought fit to bring forward. The testimony to the skill of osteopathic witnesses had the savour of advertisements of quack medicines.

Lord Elibank: You said that osteopathy in America was declining. I saw nothing in what you read that supported that contention.

Sir F. Buzzard: I have, as I said, travelled in America a great deal, and talked with Americans about cults, for that is their happy hunting ground, and the impression I have gained is that though at one time osteopaths were popular there, they have now taken second place to chiropractors.

Mr. Thorpe, K.C.: Matters are now being stated which have never been put to my witnesses in cross-examination. This is a most material allegation.

The Chairman: I understand that the witness is not represented by counsel.

Mr. Thorpe: If he is to be an "unlicensed libertine" in the matter of an advocate your lordship will be allowing a departure from the ordinary practice.

The Chairman: The point will be noted.

Lord Elibank: I do not see how the quotation from the book supports the witness's conclusion.

Sir F. Buzzard: It speaks of osteopathy as the forerunner of chiropractic.

Lord Elibank: It depends what "forerunner" means.

Sir F. Buzzard next discussed the limitations whereby under the Bill an osteopath would not administer general anaesthetics, nor perform major surgery, nor practise midwifery. A student who was ignorant of anaesthetics and their effect upon the tone of muscles would be dangerous as a manipulator; one who knew nothing about obstetrics and the structural changes brought about by pregnancy should never be entrusted with the manipulation of a woman, and a student who had not studied major surgery was not in a position to know when the skill of the surgeon was essential to his patient's survival.

State Recognition and the Curriculum

It was proposed, the witness went on, to add to the Board to be set up under the Bill one registered medical practitioner appointed by the General Medical Council. This added enormously to the danger of the Bill as regards the public. Assuming for the sake of argument that the General Medical Council was willing to appoint registered medical practitioners to the Board, the public would gain the idea not only that there was State recognition of osteopathy, but that there was

recognition by, what might seem equally important, the medical profession. He could not believe that the medical profession would lend itself to a step so retrograde and so dangerous to the interests of the public.

Sir F. Buzzard next pointed out that in the Bill an osteopathic student had four years in which to acquire that amount of knowledge which a medical student had to acquire in six or seven years before he qualified. No provision was made for the study of biology, physics, and chemistry.

Lord Esher said that chemistry was in the Bill.

Sir F. Buzzard said that that was chemistry on the organic or biochemical side. No time was provided for the elements of chemistry, physics, and biology, a knowledge of which was an absolutely essential preparation for the understanding of anatomy, physiology, pathology, medicine, and surgery. The difference in length of training between the medical student and the osteopathic student was more like three years than one year. Osteopaths insisted on a very high degree of skill and experience in their special methods of treatment, and if that was to be attained during the four-years course it must be at the expense of what was far more important, the acquirement of a knowledge of disease such as would make the student efficient in diagnosis. Incidentally the witness protested against the term "orthodox medicine." There was no such thing. There were orthodox principles in medical education. Throughout the curriculum students were invited to observe by means of experiment and in other ways what were the ascertained facts with regard to the structure and function of the body. He was made acquainted with a number of theories, many of which could not be proved to the satisfaction of everyone, but he was not asked to commit himself to any. Most effort was directed to making the student skilful in diagnosis, the most important equipment for anyone in the practice of medicine.

Misunderstanding appeared to be rife with regard to the time and labour spent by students in learning about drugs. Special instruction in that subject was limited, and the student gathered his knowledge of drugs mainly from observing how and when his teachers employed them. He was expected to be well versed in the use of drugs which were specifics. But the medical profession had long ceased to regard drugs as the most important factor in maintaining health and combating disease; it had even reached that position long before he was a medical student.

Quality of Teaching and Research

The real criterion of training (Sir Farquhar Buzzard continued) was the quality of the teaching and of the research on which it was based. Here he drew attention to *The Principles of Osteopathy*, by Yale Castlio, D.O., professor of the principles of osteopathy and supervisor of the clinics, Kansas City College, widely quoted by the *British Osteopathic Review* as an authority. Professor Castlio wrote:

> "My experience in studying the principles of osteopathy while in college, confirmed later by experience in teaching this subject, has convinced me that a new presentation of osteopathic principles is vitally necessary. . . . We have now at our disposal a considerable body of experimental evidence, scientifically exact, conservatively interpreted."

He had studied this book with some care, especially the part which dealt with the nervous system, and had been amazed at the ignorance displayed by the author in regard to the anatomy, physiology, and pathology of the spinal cord. "The misstatements and misrepresentations are so glaring that no one who has any knowledge of the subject would waste time in finishing the book, and it would be almost criminal to put it in the hands of a medical student." He instanced one among many examples—a photograph of a section of the spinal cord, described as "illustrative of the effects of the osteopathic lesion on the spinal medulla." The description went on: "Area near the tip of the posterior horn. Note beginning of degeneration of nerve cells, pericellular space, swollen axis cylinders, several hyperaemic vessels, and the cell infiltration." In fact, it was not a photograph of the posterior horn at all, but of the base of the anterior horn—a fundamental point which any student of anatomy would appreciate. Of the degeneration of cells there was no evidence, the pericellular spaces were not pathological at all, there were no swollen axis cylinders. The whole thing, coming from a research institute, exemplified gross ignorance of the anatomy and pathology of the spinal cord.

No doubt he would be asked why more had not been done to investigate the claims of osteopathy to scientific recognition. The sciences of physiology, anatomy, pathology, and medicine covered an enormous field, in which thousands of well-trained researchers were working all over the world. There were hundreds of theories put forward with prima facie evidence sufficient to justify further inquiry. What was the prima facie evidence put forward in support of osteopathy? He had indicated the worth of the scientific evidence. He had voluntarily listened to a distinguished osteopath demonstrating the theory of manipulation, but had not been convinced that the claims could be substantiated. A large number of his patients had consulted osteopaths before they came to him. The patient said, "And then I went to an osteopath, and he told me——"; and there he always interrupted and said, "And he told you there were two or three vertebrae wrong and he would put them right," and the patient would reply, "That is what he did say. How ever did you know that?"

"When that is one's experience one is not prepared to take up osteopathy as being in any sense a new and valuable system of healing art. Nor can one be impressed by a system which tells the student from the very beginning of his career that osteopathic theory and treatment must be adopted. In medicine we do nothing of that kind at all. We do not teach the students anything except what can be demonstrated to the full. We put the theories before them, but do not ask them to adopt any."

The committee adjourned until Friday, March 29th.

BRITISH MEMORIAL TO MARIE CURIE

A DAUGHTER'S TRIBUTE

A banquet to support the British memorial to Marie Curie was held at Claridges on March 20th, under the chairmanship of Mr. Neville Chamberlain, Chancellor of the Exchequer. The immediate purpose of the fund which is being raised is to provide £50,000 for the endowment, extension, and maintenance of the Marie Curie Hospital, Fitzjohn's Avenue, Hampstead, and an additional income for that institution of £5,000 a year. Mr. Chamberlain and the Countess of Minto, president of the memorial, received the guests, who included the French and Spanish ambassadors and distinguished representatives of medicine and physicial science.

Mr. Neville Chamberlain, who said that he had been asked to preside owing to his long association with the Ministry of Health, paid what he described as a humble tribute to a very great woman, a most original and fertile investigator, and one of the great benefactors of the human race. Her accomplishments, which would have been wonderful in any man, seemed almost miraculous in a woman—a remark which was greeted with some laughter and gentle protest. Madame Curie, he said, owed nothing to wealth and influence; she was gifted with great powers of mind, and had a passionate devotion to the pursuit of knowledge. He mentioned her lavish and self-forgetful gifts—she presented to the Radium Institute in Paris the whole of the radium salts which she and her husband had extracted from pitchblende, and later gave to the same body a gram of radium which had been presented to her personally by the women of America.

The toast "The Memory of Madame Curie" was responded to in a very felicitous speech by one of her two daughters, Mlle Eva Curie, a well-known musician. She said that her mother often repeated the words, "In science we should be interested in things, not persons." The words on her lips meant chiefly that her own person was of no importance compared with the things to which she devoted her life. A discovery made by any scientist gave her as sincere joy as a discovery of her own. Tributes paid exclusively to her personal glory were a sort of torture to one of her extreme timidity and natural reserve. The burden of celebrity overwhelmed and oppressed her, and at the end of her life she had still to say, "I am nothing but a student." Those responsible

for the Marie Curie memorial seemed to have understood this, for they had paid to this modest woman the only tribute she would have liked and accepted. They had given her name to a beautiful and generous work. In the simple name of the Marie Curie Hospital she saw the epilogue of a fairy tale which started in 1891, when a poor Polish student came to Paris to learn about the most abstract and severe things to which a woman ever got devoted. Marie Curie gave up her rule of life to be interested in things, not persons, when she knew that there were thousands of people who were suffering and whom she could help. All the passion that she scorned to devote to the seeking of honours she devoted to helping those who needed her, and whom she had suddenly the marvellous power to relieve. Mlle Curie spoke of her mother's work during the war, when she drove radiological cars on the French and Belgian fronts, and carried out the *x*-ray examination of thousands of wounded, and of her post-war activities in the practical development of radium therapy.

"Marie Curie lives no more. But in a hospital which bears her name, surrounded by the trees and the flowers that she liked so much, women scientists, prominent women doctors, are now using the power of radium to cure other women who suffer and who need help. Some of these patients do not even know, perhaps, who was my mother, nor what is radium. It does not matter, and it is part of the fairy tale. Thanks to Marie Curie and thanks to you, these patients will be relieved from their pains, and often cured. The creation of hospitals like the Marie Curie Hospital was one of my mother's greatest dreams. I am happy and grateful to think that it is in England, in a great country which my mother admired and loved, that that dream of hers has been realized."

After a speech so moving, the remainder of the oratory was in danger of being anticlimax, but it was worthily sustained by Sir William Bragg, who spoke of the eminence of Madame Curie in physics, and of the difficulty and hardship of the conditions under which her discoveries were made—in a building with an asphalt floor and a glass roof, by no means proof against rain, a hothouse in summer, and in winter scarcely warmed by a single stove, with furniture consisting of old deal tables on which the specimens were deposited. But Madame Curie wrote, "I shall never forget the ravishing joy we used to have when, on coming in at night, we saw the faintly luminous shapes of the produce of our toil."

Miss M. M. Chadburn, chairman of the hospital, anticipated the possible criticism: Why multiply small hospitals? She pointed out that the Marie Curie was linked up with four general hospitals and was recognized by the Medical Research Council as a centre for treatment by radium. It had eighteen surgeons who had agreed to co-operate in this work, all using the same technique. As to results, she mentioned several matters of encouragement, including the fact that the two first patients treated in the hospital, which was founded in 1929, were alive to-day, free from any signs and symptoms of disease. Miss Addison Philips gave expression of gratitude to the donors, and amused the company with some epigrams with a feminist bias, but she added that women had undertaken this work at the Marie Curie in no spirit of ostentation or mean emulation, but only with the desire to extend the boundaries of knowledge and relieve suffering. Mrs. Walter Runciman proposed the health of the chairman.

At the close of the proceedings it was announced that a sum of £3,857 had been so far collected or promised for the fund. It was also stated that the expenses of the banquet had been defrayed anonymously, so that the whole amount given would go direct to the purposes of the memorial.

The seventh International Congress on Industrial Accidents and Diseases will be held in Brussels from July 22nd to 27th. The chairman of the British Committee is Sir Thomas Oliver. Full details of the congress may be obtained from the secretary of the Industrial Welfare Society, 14, Hobart Place, S.W.1.

Scotland

Incapacitating Sickness in Scotland

The fourth of a series of annual summaries of the principal features of incapacitating illness among the insured population of Scotland has been issued by the Department of Health, and deals with the year July 1st, 1933, to June 30th, 1934. The report is favourable in that the number of completed incapacities is appreciably less than the number for the previous year; in some respects the report is the most favourable since the compilation of these summaries began four years ago. The greater part of this improvement is due to the diminution in the number of cases of influenza, for incapacities attributed to this cause were only 35,652 as against 109,462 in the preceding year, while days of sickness attributable to influenza were 1,092,429 fewer. There was a decrease also in bronchitis and pneumonia. The decline in these, however, is set off to some extent by definite increases in tonsillitis, scarlet fever, erysipelas, diphtheria, acute rheumatism, and superficial inflammatory conditions. Another favourable feature of the report is the fact that among married women the incidence of incapacities has over the past four years consistently improved in contrast with single women or men. In the last three years there was an average annual diminution of 2,000 cases of incapacitating diseases. An attempt has been made in this report, for the first time, to study the complex field of occupational morbidity by an investigation of the disabilities of miners. With regard to the incidence of incapacity in 1930–1, there were 224 cases per 1,000 of the insured population, which by 1933–4 had decreased to 202, a reduction of over 15 per cent. The total number of cases in the earlier year was 378,742, and in 1933–4 it was 341,046 out of an insured population of 1,689,468. The rate per 1,000 in 1933–4 was 190 cases among males and 226 among females. The number of days lost per person during the year was for both sexes 9.99—for males 9.19 and for females 11.62—the total lost by both sexes together being 16,870,515. The average duration of incapacities was for both sexes together 49.47 days—for males 48.32 and for females 51.47, each of these showing an increase over the figure for the previous year. An interesting feature of the report deals with the incidence of incapacitating sickness in different occupational groups. There was a considerable excess of incapacitating sickness among those employed in mining. Textile and transport workers and makers of foods and drinks came a considerable distance below miners. On the other hand, workers in metals, in the building trades, and those employed in commercial occupations and in agriculture and fishing showed an incidence of sickness less than half that of miners. The lowest incidence was among domestic servants, chauffeurs, etc. A very substantial difference thus exists between the sickness rate of miners and that of other males in Scotland. On the experience of three years, from July, 1930, to June, 1933, the annual average duration of incapacity per miner was 18 days as compared with 8.7 days for other men. This was produced chiefly by a much higher frequency rate of sickness than by longer duration of individual incapacities. Thus among miners there were 405 cases of sickness per 1,000 miners during this period as against 190 for all other males in Scotland. Concerning the causes of incapacity, miners show better health than the average in respect to tuberculosis, malignant tumours, diabetes, infectious diseases, cerebral haemorrhage, and nervous debility; in all other specified causes the miners' rates of sickness are in excess.

St. Mungo's College Medical School

At the closing meeting of the winter session of St. Mungo's College Medical School, Glasgow, Sir James Macfarlane presided, and Dr. John Patrick, F.R.C.S.Ed., presented the prizes. Dr. Patrick, in his address to the students, said that the extramural schools in Edinburgh and Glasgow—that is, the schools outside the universities—should be preserved and encouraged, for many reasons. Strong extramural schools had tended to keep up the standard of teaching in the universities, and they had often formed a training ground for those who later became professors in the universities. He criticized the methods of study during the student's course and the qualities which made for success in the medical profession, and said that there were many people, the loose-thinking, incoherent-talking type, in this country who would like them to believe that the whole profession of medicine was a failure. Nevertheless, if they looked back on the work and lives of men like Hippocrates, Harvey, Jenner, John Hunter, Lister, Macewen, and others they could not but feel that they had no right to allow time to be wasted by osteopaths, chiropractors, Christian scientists, naturopaths, and the cults of other cranks.

Exchange of Lecturers between St. Andrews University and Basle

At a meeting of the University Court of St. Andrews University on March 20th it was intimated that Professor Waymouth Reid, of the chair of physiology in University College, Dundee, was resigning his chair, and arrangements were made for the selection and appointment of a professor to succeed him. The Court approved an ordinance founding a chair of geology in the University. It was also intimated that an arrangement had been made for an exchange of professors between the University of St. Andrews and the University of Basle, Switzerland, under which a course of lectures was delivered in the St. Andrews Medical School by Professor Doerr of Basle, and a course of lectures was given in Basle by Professor W. J. Tulloch of the bacteriology department in the medical school at Dundee.

Glasgow Royal Cancer Hospital

At the annual meeting of the Glasgow Royal Cancer Hospital, held on March 15th, Sir Robert Rait, Principal of Glasgow University, who presided, said that the diagnostic clinic, which was inaugurated early last summer, had resulted in a large number of cases of cancer being treated at an early stage, when eradication might reasonably be expected. A number of extensions had been effected at the hospital, and the new laboratory, which was now in use, had greatly improved the facilities of the pathological and research departments. An important development had been the installation of a new deep *x*-ray therapy apparatus, which had added to the safety of the patients and their comfort while under treatment. He welcomed the fact that the hospital was acting with the British Empire Cancer Campaign, because the best chance of success came from combined effort and from constant intercommunication between different institutions doing this work. Dr. Peacock, director of research in the hospital, said that some progress had been made in the direction of improving their knowledge of the causation of cancer. Their difficulty was to get patients at an early stage. In the therapy department, however, even some of the worst cases had given surprising results; in a few life had been prolonged for many years, so that the patients ultimately died of another disease. The patients in the latter cases might be regarded as cured. With regard to the finance of the institution, it was reported that the ordinary income had been £8,109 and the ordinary expenditure £12,905, leaving a deficit of £4,796. This had, however, been met from extraordinary income, with a surplus of £4,053, and after expenditure on alterations and other capital charges there remained a net addition to the funds of the hospital of £1,630.

Glasgow Hospital Sunday Fund

At the annual meeting of subscribers to the Glasgow Hospital Sunday Fund, held on March 20th, the Lord Provost directed attention to the gradual decline in the amounts contributed through this agency over the past ten years. The total collection for the past year amounted to £8,201, a decrease of £322 as compared with the sum for the previous year. After meeting administrative expenses £7,890 remained to be allocated to the three infirmaries in proportion to the number of their beds as follows: Royal Infirmary (856 beds), £3,513; Western Infirmary (630 beds), £2,584; Victoria Infirmary (437 beds), £1,793. The organization of Glasgow students, he added, had collected over £10,000 in one day, and he thought that the churches might again bring up their contributions at least to the same figure.

England and Wales

Mental After-care

The annual meeting of the Mental After-Care Association was held at the Incorporated Accountants' Hall, London, on March 22nd. Lord Wakefield, who presided, said that last year 3,190 patients had been helped, an increase of 325 on the previous year. Some idea of the magnitude of the work might be gathered from the fact that considerably over 9,000 visits were paid during 1934, and between 600 and 700 patients had been received for convalescence. A new branch of the association's activities was the pre-care of patients who submitted themselves for preventive treatment. The work in that direction continued to grow, but the association would welcome the opportunity of doing still more. Mental illness need no longer be faced with the old despair. Many wonderful recoveries, as the annual report stated, had been secured, thanks not least to the association which had eased the return to normal life, giving the patients a breathing space during which they gained heart and courage for the new adventure. Dr. Reginald Worth, chairman of the council, submitted the report, which referred to the increasing number of applications for help received from mental hospitals, public assistance committees, and other official bodies. Acting under Section 55 of the Lunacy Act, the association had been actively boarding out a certain number of men and women who would never face the stress of outside life except under supervision and a certain amount of guardianship. The financial position of the association had steadily improved, thanks to generous donations from Lord Wakefield and others, and a contribution of £600 per annum which the London County Council had agreed to make towards the expenses of patients sent from its mental hospitals. Sir Hubert Bond, in moving the adoption of the report, spoke of the large field still awaiting the association if it received the support it deserved, and Dr. H. Yellowlees, in seconding, said that he had read one sentence in the report with dismay: "Ours is always a difficult work in which to interest the public." Well, he was sorry for the association—but especially for the public. One of the most valuable features of the work of the association was the help given at that critical stage when the patient was well again in the

medical sense, but dazed and confused and terrified of a recurrence of his malady. Sir Walter Langdon-Brown proposed the re-election of officers and council, and referred with great regret to the death of Sir Maurice Craig and others long associated with the work. Brief speeches were also made by Sir Thomas Keens, who spoke of the lack of formality and officialdom in this valuable public service, and by Dr. W. G. Masefield and Dr. W. D. Nicol.

District Medical Officers in London

The London County Council has had under review the methods in operation for the provision of district medical services. At present the work in ninety-six medical relief districts in London is performed by part-time district medical officers, and in ten hospital districts by the staff of the appropriate hospitals. The Hospitals and Medical Services Committee reports that some time must still elapse before the Council will be in a position to arrive at a final decision as to the method of service best suited to the needs of the sick poor. Therefore it is proposed that the existing temporary engagements of district medical officers be continued for a further year, and that any further engagements which it may be necessary to make should also be on a temporary basis and operate until March, 1936. An exception is proposed in one area (Deptford and Lewisham), where, in order to permit of the trial during the ensuing year of possible alternative experimental arrangements which are being considered, the engagements are to be extended for a period of six months only. In view of the uncertainty as to the policy to be adopted in the future with regard to the organization of the district medical service, the committee considers that it would be undesirable at this stage to attempt any final determination of the salaries of part-time district medical officers. It is understood that the Public Assistance Committee proposes to make considerable modifications in the relief districts which are necessary as a result of the Unemployment Assistance Act, and this will entail corresponding changes in the medical relief districts. The provisional salaries are, however, being adjusted in some cases in the light of statistics of dispensary attendances and domiciliary visits during the period April 1st, 1933, to December 31st, 1934. This review has been made in the light of a scale based on the estimated daily time required for the work in visiting patients in their houses and attending the dispensary. Allowance has also been made for the fact that in certain cases the medical officers are required to see patients at their private surgeries and not on premises provided by the Council. As a result it is proposed that increases in the provisional rates should be made in twenty districts and reductions in seven. The effect of this will be to raise the total of salaries paid to part-time district medical officers from £18,211, as at present, to £19,636 a year. The number of domiciliary visits paid by these part-time officers has increased from 38,326 in 1930 to 59,688 in 1934, and the number of dispensary attendances from 120,708 in the former to 163,514 in the latter year.

Leeds Maternity Hospital

At the annual meeting of the Leeds Maternity Hospital on March 20th the need for further extensions was voiced by the Lord Mayor and other speakers. The annual report showed that the in-patients during the year numbered 3,363, being 255 more than in 1933. The births numbered 2,478, an increase of 372, representing 40 per cent. of the births of the city. There was also a large increase in the number of out-patients. The honorary secretary, Mrs. Austyn Barran, said that while these increases showed the growing popularity of the hospital among the medical practitioners of the city and district, and among the women of Leeds and the West Riding, a certain apprehension was felt for the future. Additional premises adjoining the hospital had been bought by the board, and these now housed the administrative staff and the extra nursing staff. While the ordinary income had increased by reason of a rise in receipts from and on behalf of patients, the annual subscriptions showed a serious decrease. The chairman of the board of management said that the increase in the number of out-patients was especially gratifying because the board claimed that the value of the work done at the hospital lay chiefly in the province of ante-natal care and advice. Dr. G. W. Watson recalled the establishment of the hospital thirty years ago, when he and others could not see much of a future for it, but it had grown steadily till now it was perhaps the most important maternity hospital in the provinces. The meeting closed with a vote of sincere thanks to Dr. William McGregor Young for his long and valued services as senior honorary medical officer, a post which he relinquished last year.

Central Midwives Board

At the March meeting of the Central Midwives Board for England and Wales a letter was read from the Ministry of Health approving, for the purpose of apportionment, the balance against the Board of £4,089 8s. 9d. for the year 1934. It was announced that Miss K. J. Stephenson and Dr. J. J. Buchan had respectively been re-elected to the Board by the County Councils Association and the Society of Medical Officers of Health. A letter was read from Dr. F. H. Lacey of Manchester asking if it would be possible to hold the midwife-teacher's examination at a centre in the North of England as well as in London, as thereby candidates in the North of England would be spared the expense of journeying to London ; or, in the alternative, if it would be possible to hold the written part of the examination in the North of England as well as in London. The Board's reply was that it did not consider it practicable at the present time to arrange that the whole examination should be held at any centre in addition to London. Owing, however, to the desirability of encouraging the formation of centres at which courses of instruction in preparation for the midwife-teacher's examination will be held, the Board is willing that Manchester be a centre for the written part of the examination this year, and that arrangements for other centres in future years be made in accordance with the reasonable requirements of candidates. It was resolved that the regulations governing the admission of candidates to the midwife-teacher's examination, as revised and amended, be adopted, and that they remain in force in the years 1936–8 inclusive.

C. N. Kavanaugh (*Arch. Int. Med.*, January, 1935, p. 61) reviews the literature and records his observations on 123 cases of tularaemia, with the account of the necropsy in one. Sixty-nine of his cases occurred in males and fifty-four in females ; 108 were of the primary cutaneous type, six of the primary ophthalmic type, and nine of the cryptogenetic type of the disease. The average incubation period as definitely determined in fifty-eight cases was four and a half days. Pulmonary involvement occurred in sixteen. The breast was affected in two cases. Abdominal symptoms were prominent in five, peritonitis being present in one. Delirium was a striking feature in six. Acute mania occurred in one. Subcutaneous nodules were found in twenty-eight, and a cutaneous eruption was present in twenty-three. There were five deaths, a fatality rate of 4 per cent. The characteristic pathological processes were found in Kavanaugh's case in the form of focal areas of necrosis in various stages of evolution.

Reports of Societies

TISSUE CULTURE AND SKELETAL DEVELOPMENT

At a meeting of the London Association of the Medical Women's Federation held on February 26th at B.M.A. House, with the president, Dr. E. PICKARD, in the chair, Miss HONOR FELL, D.Sc., read a paper on tissue culture and the study of skeletal development.

Dr. Fell said that tissue culture technique was really established by Ross Harrison in his classical experiments on nerve outgrowth in the frog, and was adapted for the cultivation of tissues from warm-blooded animals by Burrows and Carrel. Tissue culture consisted essentially in removing a small fragment of tissue from the body and explanting it in a small quantity of nutritive medium in a suitable glass vessel. The best culture medium was a semi-solid clot formed by a mixture of blood plasma and saline extract of embryonic juices. The cultures were periodically transferred to fresh medium and incubated at body temperature. Strict asepsis had to be observed in all manipulations. Tissues cultivated *in vitro* showed two types of growth, termed respectively "somatic" and "uncontrolled." In somatic growth the explanted fragment grew and developed almost as if it were still part of the body, and it was with this type of growth that Dr. Fell's remarks were concerned. Uncontrolled growth was characterized by a progressive simplification of the original histological structure correlated with a diffuse outgrowth of the cells into the medium. The problems of skeletal development fell into two main groups: (*a*) histogenetic problems relating to the differentiation of skeletal tissues, and (*b*) anatomical problems relating to the differentiation of gross skeletal form. Tissue culture had contributed to the solution of both (*a*) and (*b*). Dr. Fell dealt first with histogenetic problems; one of these concerned the relative importance of extrinsic and intrinsic factors in the primary development of cartilage in the early embryo. Fragments of the undifferentiated chondrogenic tissue from the limb bud and jaw of three-day to four-day fowl embryos when cultivated *in vitro* readily formed cartilage in the complete absence of a nerve and vascular system and of the normally associated muscle. Similar explants of undifferentiated tissue from non-chondrogenic areas failed to form cartilage. These results afforded evidence that in embryonic development cartilage differentiates as a result of factors intrinsic in the cells and not in response to environmental influence. Another problem of histogenesis on which tissue culture shed considerable light was the much-vexed question of the nature and function of the osteoblast. The osteoblast was now sometimes regarded simply as an active fibroblast and not as a cell specifically differentiated for bone formation, whilst bone formation was thought to be caused merely by the presence of excess calcium in a young and highly vascular connective tissue. It therefore seemed important to discover whether osteoblasts could form bone when removed from the body and grown under the standardized conditions obtainable *in vitro*. Osteogenic tissue from three different sources—namely, the jaw rudiment, periosteum, and endosteum—was therefore cultivated. These cultures readily formed bone, but similar cultures of fibroblasts never ossified. From this it was concluded (*a*) that the osteoblasts are directly concerned with bone development, and (*b*) that there is an essential physiological difference between the osteoblast and the fibroblast.

Dr. Fell then considered the importance of tissue culture in the study of anatomical problems with special reference to the question of joint formation. Joint formation involved two main problems: (*a*) whether the characteristic shape of the articular ends is due to extrinsic or intrinsic factors, and (*b*) how two opposing bone rudiments developing from a common block of mesoderm are able to separate and form two movable, independent units instead of developing in continuity. If the early femur rudiment of the fowl was removed from the body and cultivated *in vitro* it developed a remarkably normal anatomical form with condyles, head, and trochanter. This result confirmed P. D. F. Murray's view that the general form of the bone was determined by intrinsic factors. In order to study the mode of separation of adjacent articular surfaces, the Y-shaped condensation of mesoderm representing the rudiment of the limb skeleton was removed from the limb bud of the embryonic fowl and cultivated *in vitro*. Such explants formed a femur separated by a well-marked joint from the tibia and fibula. The joint did not persist *in vitro*, but disappeared by secondary fusion. These results showed that the earlier stages of joint formation could proceed in the absence of muscular action and of a nerve and blood supply. A further series of experiments on the explanted skeletal rudiment of the leg were described, as a result of which it was concluded that the separation of the articular surfaces was the mechanical result of differential growth, although the characteristic shape of the knee-joint was intrinsic in the limb mosaic.

After the lecture a cinema film, photographed by Dr. R. G. Canti, was shown, in which the development *in vitro* of the isolated femur rudiment and of the knee-joint was demonstrated.

PULSE IRREGULARITIES

At a meeting of the Devon and Exeter Medico-Chirurgical Society, held on February 28th, with the president, Dr. F. N. SIDEBOTHAM, in the chair, Dr. C. J. FULLER read a paper on "The Diagnosis, Prognosis, and Treatment of Pulse Irregularities."

Dr. Fuller, after touching on the histology and physiology of the normal heart, dealt with abnormalities in rhythm. Intermittence of the pulse might be due either to extrasystoles or to heart-block. In intermittence a premature beat (arising either in the auricle or ventricle) caught the ventricle in too empty a state for the aortic valves to open, and one therefore had to wait for the next beat before feeling the pulse at the wrist. In heart-block, on the other hand, the auricular excitation wave was interrupted in the bundle of His, with resultant failure in ventricular contraction. Thus, although an extrasystole *per se* was not necessarily of moment, heart-block was always indicative of heart disease in a serious degree. Clinically, the distinction proved simple. In the case of an extrasystole too weak to open the valve, a first sound could be detected, but not a second. In heart-block there would be silence throughout the cardiac cycle and absence of cardiac impulse. Again, with extrasystoles sufficient in force to open the aortic valve, there would be a premature beat with audible first and second sounds. Extrasystoles could arise anywhere in the heart; when arising in the auricle a complete beat resulted, whereas in the ventricles, although ventricular contraction took place, this had the effect of arresting the normal auricular impulse (then due), and thus creating a pause. When the pulse rate accelerated to 120, extrasystoles disappeared, as a rule; a persistent irregularity at over 140 was seldom, if ever, associated with extrasystoles. Extrasystoles might occur with or without pattern; in the latter case they were "coupled," as was seen where digitalis had been pushed too far. Although it was necessary to make a rigid routine examination of the heart in all cases, extrasystoles were often merely indicative of a temporary disturbance in rhythm, secondary to faults elsewhere, such as digestive errors, constipation, pulmonary affections, and abuse of tobacco, etc. As regards treatment, the bromide and luminal groups were useful where no underlying cause could be detected. Where serious inconvenience was encountered, but not otherwise, recourse could be had to quinidine. Dr. Fuller then explained the mechanism of heart-block, and stated that diagnosis of the earliest stages—that is, where there was incipient delay in auriculo-ventricular transmission—could only be established by a cardiogram. Next there followed occasional failure to evolve ventricular response, and so on, until one met with the case of complete heart-block. Where complete block occurred the ventricle might be capable of taking on a rhythm of its own, but, failing this, sudden death occurred. A

constant ventricular beat of 30 to 35 under conditions of rest and effort alike was a striking feature in cases of complete heart-block. More difficult to detect were the effects of bundle-branch block—that is, where conductivity was cut off from one ventricle alone. A gallop rhythm or accentuated second sound was a frequent concomitant, but the diagnosis could not be clinched without the aid of the cardiograph. Although heart-block following acute infections disappeared in the majority of cases, it pointed to serious mischief in the myocardium so long as symptoms were present; but when permanently established (whatever might be the degree) the prognosis was very grave, especially when associated with cardiac enlargement, pain, and nocturnal distress. Disease of the coronary arteries and syphilis were common aetiological factors in the permanent group. With cases giving a positive Wassermann reaction treatment lay in the iodides, mercury, and arsenic—the last-named with caution. For the rest one could only treat symptoms as they arose and on general lines. Atropine, adrenaline, or barium chloride (1/2 grain daily) all had their uses in the syncopal attacks, but digitalis and strychnine were contraindicated. The speaker then passed on to auricular fibrillation. Persistent auricular fibrillation was found in rheumatic fever and chorea, but rarely before the age of 12. It was also a frequent feature in mitral stenosis, but rare in aortic disease. Other associations were coronary arterial disease and hyperthyroidism. Paroxysmal fibrillation was a not infrequent complication of the acute infections, and was seen also in hyperthyroidism, but the initial attack could only be diagnosed after its cessation. The prognosis for that particular infection was relatively graver when fibrillation was associated. Permanent fibrillation was always of serious portent as regards expectation of life. On the other hand, treatment by drugs proved of more material benefit than was the case with other irregularities, and it could be said that the outlook was more hopeful with congestive failure and auricular fibrillation than where there was failure with regular rhythm. It was not advisable (except in cases of great urgency) to give digitalis in the early stages, for an otherwise temporary condition might be so converted into a permanent one. Quinidine was the drug *par excellence* at this stage, and digitalis in well-established cases. Massive doses of digitalis could be given—with the proviso of dose supervision where immediate action was indicated—and subsequently reduced to a "maintenance" figure as the pulse rate responded to treatment. Coupled rhythm, and a ventricular rate slowed to 40 to 50, were immediate indications for suspending digitalis treatment.

A useful and stable preparation was now supplied under the name of digoxin, and could be given either by mouth or hypodermically. On an average a single "mass dose" of this could be 1.5 mg., and the maintenance dose 0.5 mg. per diem. Strophanthin intravenously was frequently dramatic in effect where there was more call for immediate action. Quinidine required to be better understood in its prescribing; the fears it inspired would then have little reason for support. In a long-standing case, or in one where there was congestion, there was absolute contraindication, as in such an instance there was the danger of conversion of auricular fibrillation into rhythmic contraction, and the launching of an embolus into the blood stream. To test susceptibility it was wise to start the dosage at 3 grains and gradually work up to the full course of 6 grains thrice daily and twice during the night. The drug should be withdrawn in any case after three weeks, as any good that was to be obtained should have been achieved by that time. Auricular flutter was relatively rare, and in this one had to consider an auricle contracting at the rate of about 300 per minute, with a ventricle seldom able to respond except at half this rate. With the irregular pulses (less commonly noted) it was difficult to differentiate from fibrillation, but, as in the latter, digitalis was the best sheet-anchor in treatment, the *modus operandi* being the conversion of flutter into fibrillation. When the latter was obtained the withdrawal of digitalis would often herald in a return to normal rhythm. Paroxysmal tachycardia was thought to be due to the superseding of the sino-auricular node by some other focus in the auricle or ventricle which was not under the same rate control. As with flutter, the rate was constant for the individual, but ventricular contraction and pulse rate were more rapid. In differential diagnosis a regular pulse rate of 160 to 200 was probably paroxysmal, while the same below 160 was probably flutter. There was usually some dilatation coincident with the attack, but where it occurred in a previously damaged myocardium acute congestive failure might develop during the attack. For the treatment of this condition vagal stimulation and rest were indicated, and quinidine could be given during the attack and as a prophylactic. Pulsus alternans was a condition little understood, but it spelt a myocardium too feeble to repeat a strong beat, and a heart working up to the very limit of its capacity. The sphygmomanometer (auscultatory method) made its recognition particularly easy; once it was established the prognosis was exceedingly grave. In conclusion, Dr. Fuller stated that it had been his aim to show how far the commoner irregularities could be diagnosed with such aids as the stethoscope, the sphygmomanometer, and, above all, personal observation of the patient. In this way diagnosis could be arrived at with fair accuracy in 90 per cent. of these irregularities. In saying this, he did not wish in any way to decry the value of the electrocardiograph where doubt was attached to the diagnosis.

In the discussion which followed Dr. CHARLES SEAWARD related his experiences with digoxin, the advantages, as they appeared to him, being that the action was immediate, and that once achieved the dose could be lowered or substituted with a maintenance dose of digitalis. He quoted a case where the initial dose had the effect of lowering the pulse from 140 to 80, and he now adopted digoxin in preference to either digitalis or strophanthus in auricular fibrillation. He remembered a case of complete heart-block where the patient, a man of about 85, had been under specialist observation on account of cardiac symptoms for sixty years. Dr. J. HEAL asked if it was advisable to push quinidine in massive doses in instances where it was not feasible to pay more than two daily visits. Dr. G. M. GREIG instanced a case of heart-block where during the syncopal attacks the pulse rate fell to 4 per minute. Dr. L. H. MOORE had had a similar experience, but in his case the block was considered to be complete. During the syncopal attacks only 5 beats per minute could be appreciated at the wrist.

Dr. FULLER, in reply, stated that the expense of digoxin was the only drawback to its extensive use. It was certainly unsafe to prescribe quinidine in massive doses unless under constant supervision. Syncopal attacks were often associated with the transitional period between partial and complete heart-block, and in these attacks atropine might prove useful.

HEART AND MIND

At a meeting of the Medical Society of Individual Psychology on March 14th, with Dr. J. C. YOUNG in the chair, Dr. G. E. S. WARD read a paper entitled "Heart and Mind."

Dr. Ward said that in his study of cardiology he had always been impressed by the supreme importance of the state of mind of the patient. An example was of one patient who believed that the electrocardiograph was a form of treatment, insisted upon its periodic repetition, and, though she had a well-marked organic lesion, was appreciably better after each record. Great benefit could come from a suggested optimism. Fear played so sinister a part in many cases that considerable reticence was necessary in giving information to the patient. The murmur was too often treated as a bogy by the medical man and, through him, by the patient. Disordered action of the heart (D.A.H.) was an obvious field of operation for the psychologist. Every step should be taken to allay the fears of those suffering from sinus arrhythmia, and it was reprehensible to call attention to it if it had not been

noticed by the patient. Premature systole was a great offender in shaking morale, and the business of the physician should be to persuade the patient of its unimportance. Heart cases usually took anaesthetics well ; the sense of dread was lost with unconsciousness and the heart action was quieted. It might not be fair to lie to a patient, but it should be remembered that a bad prognosis might be wrong and its immediate effect might be fatal. In no condition was the psychological factor more evident than in angina pectoris, a disease in which prognosis was very difficult though the electrocardiograph did help. In this condition sedatives were more useful than stimulants. In the majority of instances a missed beat at the wrist did not mean heart-block but premature systole. The most difficult case was that of organic lesion with superadded functional disturbance, and this was so not only in cardiac cases but also in patients with high blood pressure, so many of whom become obsessed by its real or imagined danger. Reassurance with the establishment of mental calm was vastly more important than attempts to reduce the pressure by drugs of uncertain action. The conclusion was that in the treatment of cardiovascular disease it was often of greater importance to weigh up the effect of the disability on the patient's mind than to tackle the cardiac or vascular abnormality alone.

DIVERTICULOSIS AND DIVERTICULITIS

At a joint meeting of the Liverpool Medical Institution and the Manchester Medical Society, held in Liverpool on March 7th, with the president, Dr. C. O. STALLYBRASS, in the chair, Mr. JOHN MORLEY (Manchester) read a paper on "Diverticulosis and Diverticulitis of the Duodenum and Colon."

Mr. Morley first described the morbid anatomy of duodenal diverticula of the common acquired type, and then gave details of six cases, on four of which he had operated. Five of the six patients had other lesions (gastric ulcers two, duodenal ulcer one, gall-stones two), which were more probably the cause of their pain than the diverticula. After considering some larger series of reported cases the conclusion was drawn that duodenal diverticula do not as a rule produce definite symptoms unless either unusually large or grossly inflamed. The anatomical features of diverticulosis of the colon were next described, and the mode of formation of the diverticula discussed. This was ascribed to the normal mass peristalsis of the colon, forcing small herniae of mucosa through the vascular gaps in the muscle coat of the bowel. Mr. Morley believed that the areas of rigid bowel wall presenting the jagged, saw-edge radiological picture described by Spriggs and Marxer as the "pre-diverticular state" are really due to an interstitial inflammatory lesion that is secondary to early diverticulosis. In a discussion of the pathology of acute and chronic diverticulitis of the colon it was pointed out that the histological features of the latter placed it in a group of lesions described in the recent German and American literature as non-specific granulomata of the intestine. A series of thirty-seven cases of diverticulitis of the colon was analysed, sixteen of which were acute and nineteen chronic. The main difficulties of diagnosis were to distinguish acute diverticulitis from acute appendicitis, and chronic diverticulitis from carcinoma of the colon. Where a colo-vesical fistula formed the probability was strongly in favour of diverticulitis rather than carcinoma. In the treatment of acute perforative diverticulitis diffuse peritonitis demanded immediate surgical intervention, while a localized abscess required a more conservative attitude on the part of the surgeon. In chronic diverticulitis the treatment was generally medical, and operation was only called for when symptoms of chronic intestinal obstruction arose that would not yield to non-operative measures. In that event radical excision was rarely feasible, but colostomy well above the point of obstruction had to be performed.

Mr. J. B. OLDHAM said that as an enthusiastic user of spinal anaesthesia he had frequently noted that when segments of the pelvic colon were in spasm the mucous membrane bulged through the muscular coat as a string of tiny blebs in the lines in which diverticula occurred. Everyone recognized that diverticulosis was infinitely commoner in the pelvic colon than elsewhere, but he wondered whether Mr. Morley could confirm his (Mr. Oldham's) experience that after the pelvic colon the region of the hepatic flexure was the commonest site for the condition.

Dr. S. W. PATTERSON agreed that there were often pathological conditions of stomach and gall-bladder, such as peptic ulcer and gall-stones, associated with duodenal diverticula. The diverticula could develop to a large size without producing symptoms ; but in about one-fourth of the cases symptoms did occur, in the absence of any associated lesion on most careful investigation, and were believed to be due to the diverticula. In regard to the nomenclature of the various *x*-ray appearances of diverticula of the colon, the palisade appearance was produced by inflammatory processes, diverticulitis. In a series of cases observed over years it had been possible to demonstrate a pre-diverticular state, characterized by rigidity of the bowel wall and interference with peristalsis, at areas which later showed diverticulosis.

Dr. R. E. ROBERTS said that he agreed that the unexpected *x*-ray finding of duodenal diverticula sometimes tended to distract attention from other less obvious lesions in the region of the duodenum and gall-bladder, the symptoms being attributable to such lesions rather than to the more obvious diverticula. He was interested in the possibility of intestinal diverticula giving rise to toxaemia from absorption of infective products from food stagnating in diverticula of the colon. He quoted one case in which a severe and intractable sciatica completely disappeared after the *x*-ray finding of extensive diverticulosis of the colon, which was then treated by liquid paraffin and guaiacol carbonate. Dr. Roberts felt sceptical about the saw-like, spiky, or palisade appearance of the colon being correctly attributed to a pre-diverticular condition ; he had found similar appearances in the presence of ulcerative colitis, and in carcinomatosis and sarcomatosis of the peritoneum. In the differential diagnosis of simple and malignant stricture of the colon the presence of multiple diverticula, shown in a barium meal or enema examination, might be misleading if they were regarded as evidence in favour of simplicity of the stricture.

Functional Digestive Disorders

Dr. N. KLETZ (Manchester), in a paper on "Functional Digestive Disorders," said that this term implied cases in which there was actual digestive dysfunction, though no organic cause. The term was not synonymous with "neurotic." These were real disorders which caused real incapacity and suffering. A most important contribution to the knowledge of digestive disorders had been the realization of the importance of the musculature and the nervous mechanisms which controlled it and modified it. In this connexion there was the important work of Alvarez, of Cushing, and of Watts and Fulton. The concept which it was sought to establish of clinical evidence and experimental work was the interdependence of the alimentary musculature and the modification of its function by stimulation of its nervous controlling mechanism, intrinsic and extrinsic, at various levels. Functional digestive disorders might occur in relation to stomach, colon, and gall-bladder. The causes were essentially the following: inherent defect of the neuro-muscular mechanism ; irregularities of habit in relation to meals, bowel evacuation, etc., including the use of purgatives ; fatigue ; emotional and psychological influences ; toxic factors, chemicals, bacterial, and metabolic ; and allergic mechanism. Though there was no doubt as to the importance of psychological and emotional factors, it was wrong to assume that these represented the only type of cause. The symptoms produced in functional digestive disorders were not in themselves different from those produced by local organic disease, which they might closely mimic, and their differentiation was most important. These disorders were of considerable frequency, and merited study,

not merely for this reason, but because of their confusion with organic disease, and their probable liability, when oft-recurring, to lead to the development of actual organic structural change.

Dr. S. BARTON HALL agreed that the subject was one of great importance, for the reason that it was, not unnaturally, liable to receive scant attention in considerations of general medicine and general surgery—a state of affairs which was only too faithfully reflected in the teaching of students, who, their attention attracted by matters of more dramatic, or examination, significance, were apt to qualify having little or no knowledge of the functional digestive disorders which they were so likely to meet in practice. He considered that the matter of diagnosis was sometimes one of great difficulty, and deprecated the diagnosis of neurosis based merely upon a process of exclusion. In his view, before a complaint could be considered neurotic two conditions must be fulfilled: first, the absence of any definite sign of disease process; and secondly, the presence of positive psychogenic factors the investigation and treatment of which were found to exert a direct beneficial effect upon the disorder concerned. Even where both these conditions were fulfilled the case might eventually prove to be organic in nature, and he cited a clinical case in illustration of this. Dr. Barton Hall advocated a closer study of the relation between constitutional types and the types of disordered digestive function, suggesting that the factor of constitutional idiosyncrasy was of almost equal importance to that of scientific dietetic principle.

ADDISON'S DISEASE

At a meeting of the Pathological Society of Manchester, held on March 13th, Dr. JOHN F. WILKINSON opened a discussion on the clinical and therapeutic aspects of Addison's disease. Dr. Wilkinson referred to the main symptoms and signs of the disease, and indicated their relative importance in diagnosis. It was shown that pigmentation was dependent upon the primary colour of the patient, and might be in little evidence in blondes until the terminal stages, prognosis being very poor in such cases. Hypotension was not always present until later, and a diagnosis of Addison's disease should not be rejected in the presence of the other symptoms and signs. Muscular weakness, general asthenia, and progressive loss of weight appeared to be earlier signs, and were out of all proportion to any obvious disease. Consideration was also paid to the presence of subnormal temperature and lowered basal metabolic rate, thyroid treatment being definitely contraindicated. The gastro-intestinal symptoms were important, and the onset of nausea, vomiting, diarrhoea, and abdominal pain was of grave significance, calling for urgent treatment. Reference was made to the occurrence of spontaneous remissions, and consequently a history of similar previous attacks of weakness, pigmentation, and gastro-intestinal upsets was frequently to be obtained. The occasional value of radiological examination and the great value of blood chemistry was stressed. It was shown that the urea, non-protein nitrogen, sodium, and chlorides in the blood during treatment furnished valuable means of control. Hypoglycaemia and marked intolerance to insulin were commonly found. The urinary changes were discussed with particular reference to the appearance of casts and albumin during crises. The value and dangers of using a salt-free diet for diagnostic purposes were considered, and also the confirmation of the diagnosis by therapeutic measures. After a brief historical survey of the production and investigation of suprarenal cortical extracts, the treatment of Addison's disease was described. It was important to use an active preparation of suprarenal cortex, and to give it in adequate quantities. The results of treatment were indicated. It was shown that at least in certain cases the employment of a salt-rich diet with 10 to 20 grams salt daily in cachets, milk, bovril, oxo, etc., enabled the maintenance dose of suprarenal cortical extract and accordingly the high cost to be very much reduced. The essential features of general treatment were outlined, particularly the need for rest, warmth, avoidance of purgation, dehydration, fatty foods, and the necessity for care in the prevention of further relapses by insisting on continuous medical supervision.

The pathological aspects of Addison's disease were reviewed by Dr. W. SUSMAN.

CARDIO-INHIBITORY FIBRES IN THE THORACIC VAGUS

At the last meeting of the Section of Anatomy and Physiology of the Royal Academy of Medicine in Ireland, held in Dublin at the Royal College of Physicians, with the president, Professor J. M. O'CONNOR, in the chair, Professor D. T. BARRY read a paper on "Cardio-inhibitory Fibres in the Thoracic Vagus."

Professor Barry said that the anatomy and functions of the cardiac nerves were generally studied as dissociated subjects. Three types of nerve had to be reckoned with from a functional standpoint—namely, efferent and afferent filaments of the parasympathetic system, and efferent fibres of the sympathetic system. From the anatomist's point of view efferent and afferent were not differentiated, and attention was given to the simple connexions of a nerve to sympathetic or parasympathetic systems. He reviewed the anatomical data, described the work of Cannon, Keng, and Perman, and stated that, below those cardiac nerves which were mostly recognizable as such anatomically, he had found that cardio-inhibitory fibres occasionally ran in the posterior pulmonary plexus. Stimulation of the vagus, cut away down to the pulmonary plexus, sometimes resulted in inhibition. The only explanation of such a result seemed to be that the inhibitory filaments are conveyed to the pulmonary vessels through the posterior pulmonary plexus.

Professor A. F. DIXON said he thought the sympathetic fibre and the vagus fibre must be quite different in the human subject from what they were in the cat or dog. One would think that in the human being the inhibitory fibres would come off lower down on the left side than on the right, but Professor Barry had found that they came off lower down on the right side. He (Professor Dixon) stated that in the human subject the superficial and deep cardiac plexuses were practically continuous. He hoped that Professor Barry had not shown that the cells of the inhibitory fibres did not originate in the brain.

The PRESIDENT considered it a trifle arbitrary to say that all excitatory fibres came from the sympathetic. He asked if Professor Barry thought that some of the inhibitory fibres he found were not from the vagus at all, but were sympathetic. He was very much interested to hear that Professor Barry could get an effect on the right auricle independently of the sinus.

Professor BARRY, in reply, said he had no doubt that the recurrent laryngeal nerve was always lower on the left side than on the right, and the fibres were always independent. If there were inhibitory fibres from the sympathetic system he thought they must be post-ganglionic.

Biochemical Methods

Dr. E. J. CONWAY read a paper on "Some Biochemical Methods." He described methods for micro-estimation of chloride and bromide and a method for estimating the alkali reserve. Using his special absorbing "unit" (described in the *Biochemical Journal*, vol. xxvii, p. 419), he treated the chloride in the fluid to be estimated with an acid permanganate mixture, which oxidized the chloride to chlorine in the cold. The chlorine diffused into the central chamber of the "unit," where it displaced an equivalent amount of iodine from a solution of potassium iodide. After ninety minutes the liberated iodine was titrated with thiosulphate, delivered from a special burette (ibid., vol. xxviii, p. 283). A drop of starch solution indicated the end-point. This method was used to estimate down to 58.5 millionth parts of a gram of chloride, reckoned as NaCl. When a standard starch solution was added to the iodide and the colour produced was compared

with that given by a standard quantity of chloride, amounts of chloride from 58.5 down to 2 millionth parts of a gram were determined. The coefficient of variation for this method was 1 per cent. Protein to the extent of 1 per cent. in the fluid analysed did not interfere with the result. Bromide was determined in a very similar manner, and could be estimated down to 4 millionth parts of a gram in the presence of thousands of times its weight of chloride. The presence of protein or other organic matter in any quantity interfered with the estimation of bromide, and it had to be removed. In the estimation of alkali reserve by the "unit," plasma was prepared by the method of van Slyke. The carbon dioxide was liberated by lactic acid and absorbed in the central chamber by standard barium hydroxide. The process was complete in about an hour at room temperature. The barium hydroxide was titrated with standard hydrochloric acid, using thymolphthalein as indicator. The volumes of carbon dioxide per cent. were calculated from a simple formula. The method was accurate to one volume of carbon dioxide per cent.

Professor W. J. JESSOP referred to the simplicity of the apparatus used by Dr. Conway, especially in the estimation of alkali reserve. He thought the methods would soon replace those now in use, which were laborious and required costly apparatus. Dr. E. HARVEY said that it was possible to do almost all estimations by Dr. Conway's methods. Professor Barry referred to the physical properties of colloids, and asked if in a case of albuminuria the estimation described by Dr. Conway would be affected, and if so, in what way. Dr. J. C. FLOOD said that unless the potassium iodide solution were made up fresh every day one pipette only must be used for it.

MEDICAL CAUSES OF SUDDEN DEATH

At a pathological meeting of the Liverpool Medical Institution held on March 14th, with the president, Dr. C. O. STALLYBRASS, in the chair, Professor J. HENRY DIBLE read a paper on sudden death.

Dealing with the causes of sudden death occurring in the circulatory system alone, Professor Dible pointed out that it was chiefly in this system that really sudden death had its cause. The commoner findings were: coronary stenosis, either with or without thrombosis, ischaemic fibrosis, aortic valvular disease, which was usually associated with myocardial disease and frequently syphilitic, and aneurysms. Among less common causes were coronary embolism, rupture of the aorta, acute myocarditis, and paramyocardial and myocardial fatty infiltration. Selected cases involving these conditions were quoted and slides shown illustrating their morbid anatomy and histology. The speaker also discussed cases of sudden death due to trivial injuries, or under conditions of great emotional disturbance, in which no lesions were to be found in the body. Many such cases occurred in comparatively young people and were associated with hypoplasia of the cardiovascular system. He regarded them as examples of acute circulatory failure, and pointed to certain analogies with acute shock. It seemed probable that the mechanism was neurogenic, and that the circulatory failure was peripheral rather than cardiac.

Mr. A. E. BURROUGHS said that a common cause of sudden death without pathological findings in middle-aged and elderly people was nightmare. He had known cases of persons subject to nightmare dying in their beds without other known causes. Dr. C. ALSTON HUGHES related that for over twenty years and in an honorary capacity he had looked after the inmates of a large home for the aged run by the Little Sisters of the Poor, and during that period he had seen many deaths among its inmates both sudden and expected. He recollected several who had passed away suddenly with no special clinical lesion. People of great age, nearing 90 or more, seem often to die of a concatenation of trivialities or merely to fade away from no disease at all but that of general senility, and to snuff out after the manner described by Oliver Wendell Holmes in his poem, the "One-Hoss Shay."

CORRESPONDENCE

Administration of Medical Services

SIR,—In the course of the preparation and leading of evidence on behalf of the British Medical Association by the Scottish Committee before the Departmental Committee at present considering the health services of Scotland there developed what I believe to be a new conception of administrative machinery for medical services. I am now convinced that, in a general way at any rate, it provides the best solution of the administrative problem, and for this reason I feel that it should early receive wide publicity and careful consideration among the general body of the Association. Briefly, the Scottish Committee advocated, first, that the medical services should be organized as regional *units* centred on the large teaching or general hospitals, each unit providing a *complete* service for the community it served; and secondly, that there should be a single administrative authority coinciding with the unit. The first point is clearly a statement of the Association's policy as contained in the General Medical Service Scheme. The second is its logical conclusion. It is so logical that one can only assume that it has not been thought of before, or thought of only to be dismissed as too simple to be practicable. I submit, however, that such an arrangement promises greater efficiency, economy, and all-round contentment than any other yet suggested, and therefore deserves the most careful examination.

It is generally conceded that the *status quo*, with its multiplicity of authority and consequent confusion, cannot be justified on grounds of efficiency. Efficiency must surely be regarded as the primary consideration. Jealousy of powers on the part of any interested body must not be allowed to stand in its way, even if this should involve financial commitments without direct control.

Hitherto opinion as to the lines on which development should occur seems to have ranged itself into two camps. There are those who think that the solution is to be found by co-ordinating existing administrative bodies, and others who see it in handing over to local authorities the entire administration of medical services. Each camp is able to bring forward very strong arguments against the other's proposals. Against the co-ordination plan there is the powerful argument that a co-ordinating body without executive power is seldom, if ever, an efficient piece of machinery. Indeed, this single argument seems to me so strong that it is scarcely necessary to adduce any others, though it may be mentioned that this arrangement would still leave unremedied many of the existing defects. The other plan at first sight seems to be the logical conclusion of recent development as embodied in the Local Government Act, 1929. There are, however, certain serious objections to it which ought to be frankly stated. In the first place, it may be said that only in a few instances would the local authority area coincide with that of the proposed medical unit, and co-ordinating schemes would therefore be necessary. A second objection is one which for reasons of mistaken politeness and short-sighted policy is seldom frankly mentioned. There exists in the profession a widespread distrust of local authority management so far as medical services are concerned. There are a number of reasons for this. Constantly public health appointments are being made by local authorities which medical men in the area know perfectly well are made as a result of considerations other than professional attainment and suitability. This objection, of course, applies not only to local authorities, but also to the large boards of

management of hospitals, dispensaries, and the like. Such a state of affairs does not make for efficiency.

Again, local authorities not infrequently irritate the profession by their refusal to listen to its advice on matters in which it is willing and competent to give advice or, where its advice is listened to, by the polite but obvious refusal to pay any attention to it. The answer invariably given to this criticism is that the local authority appoints its M.O.H. not only to administer its medical services, but also to act as its expert adviser on all medical matters within its purview. This arrangement would doubtless work well where the M.O.H. combined in his person a wide knowledge of all the work carried out in his area and of those who are doing it, the confidence and trust of the great majority of his colleagues in the area, a strong and justifiable influence with his Public Health Committee, an immunity from respect of persons, and a high degree of practical imagination and energy. The number of such paragons at present available is not great, nor does it seem likely to be greater in the future, unless as a result of deliberate policy by the inducement of commensurate rewards.

A third objection to what may be called the local authority method is a fault inherent in a system in which the administrators are directly responsible to a comparatively small electorate, composed for the most part of persons ignorant of the issues at stake. Under such a system it is almost inevitable that other considerations should be overshadowed by the financial one, more particularly that aspect of it which is concerned with immediate commitments. Even if the administrator, from his knowledge, inclines to take a long view he knows that it will be difficult, indeed well-nigh impossible, to persuade his constituents to agree with him. The inevitable result of this influence is that the examination of any proposal begins with the question not "How will this benefit the community?" but "What will it cost?"

It is for such reasons that one feels that the municipalizing of the medical services would be a seriously mistaken policy. An administrative board for each unit or area, with a small but strong personnel working under the direct control of the Ministry of Health (Department of Health in Scotland) as regards general policy, but with wide autonomy in actual administration, would be largely free from all the above objections.

Such a proposal is not so revolutionary as might appear at first sight. It falls into line with the general trend towards what has come to be known as "rationalization," and is indeed a proposal for the rationalization, with all that that word implies of economy and efficiency, of the medical services of the country. It is along similar lines that the solution of the administrative problems of such enterprises as broadcasting, electricity, and the London docks has been sought, so that we have as precedents the British Broadcasting Corporation, the Central Electricity Board, and the Port of London Authority. There are many points of similarity between these services and the medical services of the community; in fact, looked at from the point of view of our proposals, the medical service can be regarded essentially as what has come to be known as a public utility service.

Discussing this economic development of recent times, Mr. G. D. H. Cole says:

"What do these developments signify? A recognition, I think, on the one hand that the conduct of many vital services can be no longer left to the unregulated control of private enterprise, and on the other that the existing mechanism of the modern State is unsuited to the conduct, or even to the detailed regulation, of industrial and public service undertakings. There is on the one hand a desire, and even a necessity, to bring certain vital services under co-ordinated public control, and on the other an unwillingness to do this under what may be called, for short, Civil Service conditions. There is, further, in certain fields a desire to co-ordinate private enterprise without altogether superseding it; and the most obvious way of doing this may be the creation of co-ordination bodies in which the representatives both of the public and of the private undertakers who are left in the field take part."

It is probable that the study of the administration of various public utility services would assist greatly in devising a practical scheme for the administration of the medical services.—I am, etc.,

Aberdeen, March 14th. E. R. C. WALKER.

"Yaws" in the Island of Grenada

SIR,—In the course of a recent visit to the West Indies our ship put in, on February 27th last, at Port St. George, and we spent the day on the island of Grenada, one of the Windward Isles. This is a small island, but from its volcanic configuration, and the beauty of its flora and spice-bearing trees, forms one of the most interesting of this romantic group of islands. Hearing that there existed on the island a "Hospital for Yaws"—a disease at one time very prevalent among the coloured population of the island—I endeavoured to see it, hoping to learn there some details of this complaint, which is so rarely seen in England.

On my arrival, however, I found that this special hospital, which was situated in the grounds of the general hospital, and had contained a hundred beds for the reception of men, women, and children (the latter being most commonly affected with the disease), had been closed for eight years, there being no longer any need for its existence. It was, in fact, now used as a "house for the nurses" of the adjoining general hospital.

The reason for this remarkable change, as explained to me by the able medical superintendent, Dr. William Steele Mitchell, lay in the introduction of salvarsan in the treatment of the disease, which, if employed early, before the occurrence of deep ulceration and bony necrosis, quickly kills the treponema (closely resembling the syphilitic spirochaete), and leads to a cure after two or three doses. The injections of salvarsan are now usually given in the outpatient department, and, a cure quickly resulting, the beds in the Yaws Hospital are no longer needed.

At the time of my visit only a single case of yaws was to be seen in the general hospital, a coloured girl of about 14, who had not sought treatment until deep and extensive ulceration of the lower part of the leg had supervened, but which was healing rapidly after a few injections of 606. As an instance of the remarkable advance which medicine has made in so many directions in recent years, and of the striking alleviation of human suffering which has resulted, as well as the direct financial gain, this experience of the disease in the island of Grenada seems worthy of record.—I am, etc.,

PERCIVAL HORTON-SMITH HARTLEY,
Consulting Physician to St. Bartholomew's and to the Brompton Hospital.

London, W.1, March 19th.

Diphtheria Immunization: A Correction

SIR,—An annotation in the *Journal* of March 23rd (p. 594), on diphtheria and propaganda, suggests that my and my colleagues' work supports the statement that "mass inoculation . . . causes an increase of diphtheria among the inoculated." This is the same mistake which appeared in the *Journal* of June 16th, 1934, and about which, on my then drawing attention to it, you generously admitted that "owing to a typographical error inoculated was printed instead of uninoculated."

"Dudley's hypothesis" is not, therefore, as stated in the *Journal* of June 16th, 1934, and March 23rd, 1935, but is merely an inference that on occasion carriers may

arise among children who have been immunized against diphtheria and increase the risk of infection to those who are not protected—that is, children who have positive Schick reactions. The work in question was an attempt to view dispassionately the whole subject of diphtheria immunity, regardless of propaganda. It is desperately hard to write such a report and keep everything in proper perspective, because nearly every paragraph is to some extent modified by all the other paragraphs. I would be grateful, therefore, if you would print the following quotations from our much-criticized report,[1] which represent what May, O'Flynn, and I really feel regarding the value of antidiphtheria inoculation.

Regarding diphtheria in inoculated subjects: "The co-ordination of all the events which were observed in G.H.S. [Greenwich Hospital School] since 1921 leads to the belief that the relatively small extent (for G.H.S.) and the clinical mildness of the outbreak of 'gravis' diphtheria in an artificially immunized institution, where at one time 13 per cent. of the inhabitants carried 'gravis' T.C.D., is one of the best testimonials on record to the value of artificial active immunization against diphtheria" (p. 37).

I conclude the discussion on the effect of immunization on the carrier rate thus: "The author is confident that, provided an antidiphtheria campaign is conducted in a proper way, recognizable diphtheria can be quickly eradicated from any community, and the ultimate saving in health and life will far outweigh any inconvenience which may result from a temporary rise in the number of diphtheria carriers. Therefore, should perchance anyone wish to isolate a sentence from the context of the last two sections to use it as propaganda against mass immunization, the author requests he will have the courtesy to quote the last sentence also" (p. 111).

—I am, etc.,

SHELDON F. DUDLEY.

R.N. Hospital, Chatham, March 25th.

⁂ We much regret the repetition of the error.

The Nature of Acidosis

SIR,—I have read the article by Dr. Davis and Mr. Rixon on "The Nature of Acidosis" in your issue of March 16th (p. 525) with great interest, for the subject is one that has intrigued me since 1917. At that time Dr. Semon and I[2] observed the high urinary acidity and extraordinary tolerance of alkalis in patients manifesting the signs and symptoms of what we termed the "seborrhoeic state," which was rife among our troops in France. In some cases the amount of alkali required to render the urine alkaline was incredible. For example, one patient took three drachms of sodium bicarbonate and four and a half drachms of potassium citrate a day for ten days without passing an alkaline urine. We expressed the opinion that the character of the military rations and the unavoidable lack of fresh fruit and vegetables were probably in part responsible both for this high urinary acidity and for the prevalence of seborrhoeic manifestations among the troops.

We used the term "acidosis" in the sense indicated by Dr. Davis and Mr. Rixon, and, although after the war Dr. Ryffel and others failed to find any evidence of "acidaemia" in these cases, Dr. Ryffel[3] showed that the ratio $\frac{\text{ammonia-combined acid}}{\text{free acid}}$, referred to in the article under discussion, was high. Three years later Carminow Doble[4] published his observations on the hydrogen-ion concentration of the urine in the seborrhoeic state, and pointed out that, whereas that of normal controls varied from 6.4 to 6.8, in seborrhoeics the figure was 4.8 to 5.8. The conclusions at which Semon and I had arrived were subjected to considerable criticism, chiefly, I think, for two reasons—namely: (1) the absence of any evidence of acidaemia; and (2) the fact that in certain seborrhoeics the high tolerance of alkalis, which we had observed, is not present, nor does the administration of alkaline salts ameliorate their symptoms. Nevertheless, as I have shown in a very large number of cases since the war, a high alkaline tolerance is the rule in a certain type of seborrhoeic person, and is also frequently present in patients, not necessarily seborrhoeic, with a variety of symptoms, cutaneous and otherwise. The administration of alkalis in sufficient quantity in these cases often has a remarkable effect not only upon their active symptoms, but also on their physical and mental well-being.

With regard to seborrhoeics, it is possible as a rule to forecast in a given case whether a high alkaline tolerance will be found, and whether benefit will accrue from alkalis. I have for long past classified seborrhoeic patients into the "flushed" type and the sallow pigmented type.[5] The difference is well exemplified by patients with acne. The former are usually fair, robust, active, and in later life plethoric; the latter dark, pallid, thick-skinned, and hypotonic. In the former there is almost invariably a high urinary acidity, and large doses of alkalis may have to be given over a long period before the early morning urine is rendered alkaline. When this has been achieved a striking effect is the disappearance of the flushed and congested facies. In the latter the acidity of the urine and the alkaline tolerance may be less than normal; in fact, the patients often benefit from the administration of acids. It is in this type that intense indicanuria is usually found.—I am, etc.,

London, W.1, March 19th. H. W. BARBER.

[1] Medical Research Council Special Report Series, 1934, No. 195.
[2] *British Medical Journal*, 1918, ii, 245.
[3] Ibid., 1922, ii, 754.
[4] *Lancet*, 1925, i, 272.

The Report on Fractures

SIR,—Since the appearance of the Report of the Committee on Fractures which was appointed by the British Medical Association there has been considerable correspondence in your columns in regard to the various details of that report.

One point that has been discussed very fully is the investigation of the treatment of fractures at the large general hospitals. In a letter to you, Mr. Eastwood stated that the David Lewis Northern Hospital had received no inquiry in regard to their treatment of fractures, and in reply to this Dr. Gwynne Maitland stated last week that a questionary was sent to the Northern Hospital and was returned with full details of the clinic.

It seems only right that I should clear up this misapprehension. On December 29th, 1933, I received a letter from the secretary of this special committee asking various questions regarding the treatment of fractures and injuries to the limbs in the hospitals with which I was associated. This letter was addressed to me at my office, and was answered by me from there without bringing out any of the details of any one of the hospitals with which I am connected. My reply to the first question was:

"At my own hospitals we have had special fracture departments since before the war. I do not think there is much object in giving you the details of the fracture departments, but all fractures in the hospitals are treated by members of the orthopaedic staff."

I hope this will clear up this point of discussion, as it shows there really was no definite inquiry sent to the Northern Hospital, and there were certainly no full details given by me in regard to the working of the fracture clinics in any of my hospitals, as I did not think such details were necessary.—I am, etc.,

Liverpool, March 19th. T. P. MCMURRAY.

[5] Ibid., 1929, ii, 363.

SIR,—In your issue of March 16th a letter appears from Dr. T. Gywnne Maitland containing statements to which I feel a reply is necessary. He makes the suggestion that my remarks, which were meant as honest and fair criticism, had a "personal bias." This certainly was not intended, and I regret that Dr. Maitland should have seen in this letter of mine a sentiment which was very far from my thoughts. Let me assure Dr. Maitland that I not only "merely glanced at" but carefully studied the leading article in the *Journal* of February 16th, and there is no doubt that the statement is misleading.

In his reply to my paragraph which deprecated the selection of one or two hospitals for special praise he states, "It is indeed fortunate that in Great Britain we have a few hospital fracture clinics worthy of emulation." I would go further than this and substitute "many" for his word "few." Visiting other hospitals more frequently than perhaps Dr. Maitland does, I can say with confidence that there are many hospital fracture clinics run on lines similar to those advised by the Fracture Committee, and it is because of this that I still doubt the wisdom of selecting one or two as examples.

But it is on the rock of statistics that I consider the Report to founder. Dr. Maitland apparently has a wholesome respect for statistics. I, however, like many others, believe that statistics can prove anything, even the truth at times. To illustrate this, one day last week during the ordinary routine work I saw seven Colles's fractures whose average period of disability was three weeks. One was a ship's steward who returned to his full work in two hours after the setting of the fracture, three were housewives who carried out their household duties the next day, the rest were heavy manual labourers taking eight weeks or more before it was safe for them to return to their full work. What can be done with seven fractures in one day can equally well be done with a considerably larger number over a greater period of time, but these latter statistics, like the small number I have mentioned, are quite valueless unless details of occupation are also given, and even then are open to question in view of the different degrees of severity of every type of fracture.

I, like every other orthopaedic surgeon, welcomed the suggestion of a Committee to investigate the treatment of fractures, looking forward with interest to the result of their work; and it was because of my bitter disappointment to see them fall into the snare of statistics, and such statistics, that I wrote my original letter. I regret that my criticism has not been taken by Dr. Maitland in the friendly and sincere spirit in which it was meant.—I am, etc.,

Liverpool, March 25th. W. J. EASTWOOD.

Gastrectomy

SIR,—I am in cordial agreement with Mr. A. J. Walton's criticism of Mr. W. H. Ogilvie's paper on gastrectomy. It is a pity that a paper so valuable on matters of technique should be marred by exaggerated statements and condemnation of an operation which is still of great value in the treatment of gastric disease. Such pronouncements put back the clock of progress.

Mr. Ogilvie's remark that "the failures of gastro-jejunostomy meet us to-day wherever we go" seems to imply that in most cases gastro-jejunostomy is a failure. This is not the experience of all surgeons. It is doubtless true that we see more of our failures than our successes, but if we follow up our cases we see the other side of the picture. Like my friend Mr. Walton, for thirty-five years I have endeavoured to keep in touch with all my gastric patients, and except that during the years of the war a good many of these were lost sight of, I have succeeded in keeping a record of the present condition of almost all of them. For twenty-two years I have held an annual "gastric tea" at the National Temperance Hospital, to which I have invited all my gastric patients in turn. I know, therefore, that 83 per cent. of my patients are quite well, and this assuming that the untraced cases are not cured. If, as is done by some writers, the untraced cases are disregarded, the percentage of cures would be over 89 per cent.

Some years ago I analysed my results in 500 consecutive cases after gastro-jejunostomy performed more than six years previously. In that series three patients died after the operation and 89 per cent. were quite well; in 8 per cent. of the cases the results were unsatisfactory. Of this 8 per cent. of unsatisfactory results 2.6 per cent. were the result of jejunal or gastro-jejunal ulcer, 2.5 per cent. of chronic diseases such as phthisis, chronic nephritis, or cancer elsewhere than in the stomach, and 3 per cent. were due to hyperacidity. It is pertinent to inquire whether the unsuccessful cases would have been in the list of cures had some more radical procedure than gastro,jejunostomy been the operation of choice? It is at least certain that some of them would not have been alive. Lord Moynihan, Mr. Sherren, Dr. Donald Balfour, and others have also reported high percentages of cures.

I had imagined that the spectre of jejunal and gastro-jejunal ulcer had been laid to rest, but Mr. Ogilvie has resuscitated it with added horrors. This is a bogy which need not worry us. In my experience the incidence of this complication is under 3 per cent., but the important point is that it is a complication which need not arise if the patient keeps to a reasonable diet.

Mr. Ogilvie says that after gastro-jejunostomy the acid curve is often found higher than before. I do not know on what evidence this statement is founded, but I do know, from many hundreds of gastric analyses performed personally before and after gastro-jejunostomy, that the total acidity is reduced on the average by 30 per cent., and that the free hydrochloric acid, which is what really matters, is invariably reduced to a marked extent. I have never yet seen it higher after gastro-jejunostomy than before.

Another statement, of which the accuracy may be questioned, is "that gastrectomy is the only means by which the acid level can be reduced permanently below the danger point." The acidity can be, and is, reduced by gastro-jejunostomy, and, if necessary, it can be further reduced by a suitable diet. Nature's method of reducing the gastric acidity is by allowing the regurgitation of pancreatic juice and bile through the pylorus. Gastro-jejunostomy aids Nature in this endeavour by allowing a slight backflow of pancreatic juice and bile through the stoma into the stomach, and therefore is a rational, physiological, and I may add, effective operation.

Mr. Ogilvie says that "they [that is, short-circuiting operations] have done good because Nature can resist maltreatment of her normal processes." It is at least strange for one who advocates such extensive resections for gastric ulcer to refer to the simpler and far less mutilating operation of gastro-jejunostomy as maltreatment. I do not claim an intimate knowledge of Nature's thoughts, but I imagine she would prefer gastro-jejunostomy, and, if so, there may be in the years to come a rude awakening for the gastrectomists. I hope that Mr. Ogilvie himself will not be visited by the sins which he imagines cloud the lives of those who pin their faith to gastro-jejunostomy. As to Mr. Ogilvie's picturesque description of the younger group of surgeons in England and America trooping into the witness-box against gastro-jejunostomy, I would say that already in America there is a reaction in favour of gastro-jejunostomy, and I hope and believe that as the younger group of surgeons enlarge

their experience they will also gain wisdom and revert to simpler and more rational procedures.

One word more. Gastro-jejunostomy is only an incident in the treatment of gastric and duodenal ulcer. Meticulous preparatory treatment and careful and prolonged dieting after operation are no less important than the operation. It is our duty to get our patients well with the minimum of risk. The worth of gastro-jejunostomy has been proved by time, and it is still a valuable and highly satisfactory operation provided it is performed with judgement and discretion. To replace it, as a routine practice, by a more serious operation which has yet to be proved more efficacious is, as Mr. Walton says, "neither sense nor scientific surgery." "Old practices are often worth more than new theories."—I am, etc.,

London, W.1, March 22nd. HERBERT J. PATERSON.

SIR,—It may be thought impertinent for a general practitioner to air his views on such a strictly surgical subject, but my excuse is that while we can claim no thousands, or even hundreds, of cases to our lore, we do obtain a more intimate and prolonged observation of the few we see than ever falls to the lot of the surgical specialist.

In the controversy of gastrectomy *versus* gastro-enterostomy I cannot help being struck by the absence in the statistics of any mention of what, if any, medical treatment was adopted subsequent to operation, and for how long. The underlying pathology of gastric and duodenal ulceration is surely an abnormal physiological condition. Is it reasonable to expect permanently to cure such a condition by any surgical means? It is true that removal of most of the acid-producing portion of the stomach must permanently remove a hyperchlorhydria, but this, it seems, is likely to induce undesirable results of a different nature. Most surgeons seem to agree that in the presence of pyloric stenosis gastro-enterostomy is a satisfactory procedure. It is difficult to see how it can avail in the case of the ulcer high up on the lesser curvature; yet I have one such case operated on by Sir Hugh Rigby some fifteen years ago in which the ulcer was so near the cardia as to be out of reach for even a Balfour cautery excision with safety. Sir Hugh performed a simple gastro-enterostomy, and my patient has remained free from all symptoms since. Two years' medical treatment, consisting of dieting and alkalis, followed the operation.

It falls to my lot to do a certain amount of emergency surgery, and, needless to say, my commonest surgical crisis in the upper abdomen is a ruptured gastric ulcer. I have never attempted to do more than suture the opening in the stomach and establish suprapubic peritoneal drainage for twenty-four hours or so, yet the patients keep remarkably free from symptoms if only they adhere to simple medical treatment for two years after and are reasonably careful in their diet always. I have for some years used in preference to the triple powder, an emulsion consisting of chalk, bicarbonate of soda, and paraffin oil, flavoured with oil of cinnamon as a carminative. The substitution of paraffin oil for the usual carbonate of magnesia has, I think, two very distinct advantages in that it is less irritating to the colon and never gives rise to frequent loose stools, and being an oil it reduces the hypertonicity and rapid emptying usually present in these stomachs, and therefore remains longer in contact with the gastric mucosa to exert its buffer action. The prescription I finally found to be best is as follows:

Sod. bicarb.	ʒ iij
Calc. carb. precip.	ʒ x
Pulv. trag. co.	ʒ ij
Ol. cinnamomi	♏ xx
Ol. paraf. liq.	ʒ lx
Aq.	ad 1 pint

One ounce of this is taken an hour after each meal and at bedtime. It is not too unpleasant to take, and gives a greater sense of comfort than does the triple powder.

In conclusion, Sir, the points I wish to make are that the patient should not be subordinated to the type of operation but the operation to the type of patient, and that a permanently good result is more dependent on post-operative persistence with medical measures to remedy the underlying medical defect than on the exact nature of the surgical operation performed.—I am, etc.,

Rye, Sussex, March 19th. MARTIN BUTTON.

Radium Policy

SIR,—In your issue of March 23rd my friend and late colleague Mr. Bathe Rawling takes me to task for what he describes as my light-heartedness in criticizing the policy of the Kent County Council Cancer Appeal. I can assure Mr. Rawling that it was with a heavy heart, after years of experience and profound thought, that I made my criticism.

Does Mr. Rawling know that in 1929 a large sum of money was given to the nation for radium as a thanksgiving for His Majesty's recovery? The Government entrusted the control of this radium to a body of experts, who, after careful consideration, made it their policy to concentrate the radium into a comparatively small number of centres rather than to disperse it in small packets throughout the country.

Such a policy was far-seeing and sound. It will be many years before radium therapy in connexion with cancer can be standardized, and the best way of bringing about that result is to collect the patients into centres where a large series of cases can be treated under almost identical conditions, and thus the benefit of any particular technique can be evaluated. The benefit of such a policy was well illustrated in the war, when sectional interests were abolished and the treatment of certain types of wounds concentrated in definite centres. The outcome of such a policy was striking, and although one cannot hope for such immediate results in the case of cancer, where a lapse of time is necessary to prove the value of treatment, yet this very fact makes it more essential to collect the patients into large centres.

I have a great admiration for Mr. Rawling as a brain surgeon, but I suggest that he has no knowledge of, or experience in, radium therapy to justify his giving advice to the Kent County Council Cancer Appeal which cuts right across the policy laid down by the Radium Commission. If Mr. Rawling feels he must advise that more money be spent on radium therapy, I trust that he will concentrate his attention on the centre he has started at Tunbridge Wells, and thus make it into a large hospital from a radiotherapy point of view. There is considerable value in the parochial spirit, which is inherent in nine people out of ten, but in my opinion Mr. Rawling and his colleagues should have used this instinct to set up diagnostic centres in every town and village in Kent rather than spend the money on small packets of radium.—I am, etc.,

London, W.1, March 2:rd. MALCOLM DONALDSON.

Routine Paraldehyde

SIR,—Paraldehyde is closely associated in the lay mind with madness. In so far as one can assign a smell to a habitation, it is representative of the old-time asylum atmosphere as much as the odours of sanctity, or of Billingsgate, in their fields; and it is this very lingering smell that the lay visitor will carry away with him to store in some bottom drawer of memory, where

it will be associationally linked. From this angle of approach, are we being good psychiatrists when we prescribe paraldehyde as a sleeping draught to our institutional patients? When we subscribe to the paraldehyde draught round, we are, in effect, giving "knock-out" drops plus a little extra something—an undesirable counter-suggestion. Actually we are weaving an atmosphere of insanity (which remains affectionately next day), when most of all we should desire to suggest a mood of agreeableness and, so far as possible, of normality.

It cannot be denied that paraldehyde, regarded pharmacologically, is an excellent hypnotic for institutional patients, even though a few of us may suspect that it is excreted by the respiratory tract, setting up some degree of irritation. It acts rapidly and is so effective that one is tempted, time and again, to fall back on it; but to my mind its use should be confined—if used it is at all—to the acutely psychotic case who has lost insight and full awareness of his circumstances. The nightly draught is, at bottom, a lazy doctrine, even if it is not a pernicious one, and I would prefer not to employ it for a minor psychosis, or indeed any early mental disease, save in an unexpected emergency. There are other and more suitable hypnotics, whose taste can be disguised and which can be given without the knowledge of the patient.

When the need for ensuring sleep becomes imperative, it is best not to let the patient know he is being given a sleeping medicine, so that I would advocate for these psychoneurotic and early mental cases the *regular* administration, from the first day of admission, of a nightly "tea" or "soup," into which a barbiturate or similar drug can be slipped when the occasion demands. This obviates the risk of the patient becoming drug-conscious, as he might do if he were to be given a regular (or occasional) tablet or powder.

Hitherto I have utilized nutmeg "tea" (claimed by herbalists to possess narcotic properties) as a vehicle, though I am not convinced it is the best; probably peppermint "tea," or any other hot drink of an innocent nature possessing a strong and distinctive taste, would be just as good. It certainly disguises effectually the taste of any introduced hypnotic, and since it is prepared from finely ground nutmeg and hot water, it has the merit of cheapness, but that it possesses any sleep-inducing virtue of its own must be seriously doubted from my limited observations.—I am, etc.,

March 22nd. JOHN W. FISHER.

Sterilization of Women: The Legal Position

SIR,—I have only just had my attention called to the interesting letter from Mr. W. H. Stoker on this subject in your issue of March 16th. It appears to me that, although it is true that any of the acts forbidden by the Offences Against the Person Act are only criminal if done "maliciously," yet, as in the case of homicide, where such an act has been done on purpose, the onus must be on the defendant to show that he had lawful excuse. In the case of the ordinary surgical operation the law is indisputably as stated in the passage Mr. Stoker quotes from *Russell on Crimes*. But the immunity of the surgeon does not rest on the fact that the operation is surgical, but on the fact that it is done to save life or health. I take it that one is entitled (even if not a surgeon) in an extreme emergency to amputate a man's leg to save his life, on the same principle that, to use an old example, the captain may break into the cargo to save the starving crew. It cannot be disputed since the decision of Rex *v.* Donovan, cited in Mr. Stoker's letter, that the consent of the woman would not make a sterilizing operation lawful if it was done for no adequate reason; it seems, therefore, that the question whether eugenic sterilization is legal depends on the question whether the Courts will accept an indirect benefit to the community as an excuse for doing an act prima facie wrongful. In my opinion it would be contrary to general principles for them to do so: for example, to a charge of stealing whisky it would, I take it, be no defence to prove that the defendant removed it with the best intentions because he reasonably believed that the prosecutor would be better without it.—I am, etc.,

Temple, E.C., March 23rd. CECIL BINNEY.

Jochmann's Enzyme and the Pathogenic Agent in Human Bone Marrow

SIR,—In the *Journal* of March 2nd Ian MacKenzie and C. E. van Rooyen put on record interesting experiments on the pathogenic agent in lymphadenoma glands (Gordon) and in normal human bone marrow, spleen, and leucocytes (Friedemann and Elkeles; Friedemann). They particularly investigated the question whether these agents were identical with the proteolytic enzyme of Jochmann. By comparing the proteolytic and pathogenic activities of extracts from lymphadenoma glands, bone marrow, and leucocytes they arrive at the conclusion that the pathogenic agent is not identical with the proteolytic enzyme. It may be questioned whether this conclusion is fully substantiated. Although a parallelism between the pathogenic and proteolytic actions would have strongly suggested the identity of the substances, it is doubtful whether a lack of parallelism is evidence of the reverse. It may well be that some tissues contain an inhibitory substance which acts in the *in vitro* experiment but fails to do so under the very different conditions of the animal experiment.

It is, however, for another reason that I deal with the paper of MacKenzie and van Rooyen. These authors ascribe to me the theory that the pathogenic agent and Jochmann's enzyme are identical. This is, however, an error. Although I pointed out some analogies between the two substances, I emphasized the fact that their identity is not yet established. In the conclusions to my paper I wrote: "Whether the agent and the ferment are really identical remains to be finally established."[1] And a passage in the text (p. 518) reads:

> "If it could be shown that the pathogenicity of Jochmann-Lockemann extracts of human bone marrow, spleen, and leucocytes runs parallel with the ferment activity, then the identity of the pathogenic agent might be considered as established. This method of investigation has not been attempted."

I had good reasons for being cautious in my conclusions, for I had found in as yet unpublished experiments that the intrathecal injection of papain produced either symptoms within some hours or no symptoms at all. In this respect I can fully confirm the experiments of MacKenzie and van Rooyen with trypsin and pepsin. I agree, therefore, with these authors that the identity of the pathogenic agent and Jochmann's enzyme is not yet demonstrated. The distribution of both substances among the various species of animals suggests, however, that they are in some way related to each other. In this connexion I may mention new experiments, in which it has been shown that the bone marrow of monkeys (*M. rhesus*) is highly pathogenic for rabbits. According to Jochmann the monkey is the only animal which, apart from human beings, contains the proteolytic enzyme in considerable amounts. Perhaps the pathogenic agent is a split product of proteins due to the action of the enzyme.

MacKenzie and van Rooyen further showed that the treatment by which Jochmann's enzyme is extracted does

[1] Friedemann: *British Medical Journal*, 1934, i, 519.

not always kill bacteria. This, however, was convincingly demonstrated only for sporulating bacteria. With other bacteria, growth, if any, was very poor. I may put on record experiments in which it was shown that the pathogenic agent in Jochmann's extract from bone marrow stands treatment with 70 per cent. alcohol for twenty-four hours without any deterioration. From all that is known we may safely infer that non-sporulating bacteria do not stand this treatment.

I may finally mention that Mr. Barnard was kind enough to investigate a very potent Jochmann extract from bone marrow in the ultra-violet light. In visible light a number of granules were seen by dark-ground microscopy, but ultra-violet photographs of these granules did not yield images which, in form or uniformity of size, were suggestive of virus bodies.—I am, etc.,

ULRICH FRIEDEMANN.

National Institute for Medical Research,
London, N.W.3, March 20th.

Inversion of Temperature in Pneumonia

SIR,—Dr. Lacey's interesting memorandum (*Journal*, March 9th, p. 472) on inversion of the temperature in acute lobar pneumonia caused me to search for any patient in hospital with pneumonia who was a night worker. I soon discovered a night watchman, aged 64, with bilateral lower lobar pneumonia of eight days' duration, and was surprised to find that his temperature chart showed inversion. He died on the ninth day of his illness. Sputum was scanty and was not examined. Necropsy was refused. The actual temperature readings were as follows (all temperatures were taken in the mouth):

Date	Temperature in F. degrees					
	2 a.m.	6 a.m.	10 a.m.	2 p.m.	6 p.m.	10 p.m.
March 2nd ...	—	—	—	103	100.2	100.4
„ 3rd ...	101.6	102.4	101.8	100.8	101	—
„ 4th ...	—	102.4	100.4	100	99.4	100
„ 5th ...	101	102	102.4	102	99	100
„ 6th ...	100.6	100.4	99.4	98.4	99.8	99
„ 7th ...	—	101.2	100	98.2	98.6	99
„ 8th ...	—	101.2	100.8	98.6	98.2	99
„ 9th ...	—	101.4	Died 6.30 a.m.			

The fact that the temperature was inverted had been overlooked, and the temperature chart did not appear unusual until it was carefully examined. I therefore scrutinized the charts of 100 cases of lobar pneumonia, including thirty-five in which the temperature had been recorded half-hourly for long periods, but was unable to find another example of inversion or of pneumonia in a night watchman.

Most observers have found that in healthy individuals the evening temperature is slightly higher than the morning temperature. It is natural to assume that increase of temperature at the end of the day is due to the effect of extraneous conditions, such as intake of food and physical exertion during the day, and to attribute the diminution early in the morning to the fact that the body has been resting. Exceptions to this rhythm of temperature have been recorded. Dr. C. E. Lakin, in his recent Lettsomian Lectures (*Lancet*, 1934, ii, 467) on "Disturbances of the Body Temperature," said: "A reversal of the daily routine, such as occurs in night workers, leads, after a few days, to a reversal in the temperature curve." Dr. Lacey and Dr. Hingston do not say how long their patients had been employed as night watchmen. Dr. Downing's patient had been a night worker for twelve months. As far as the relatives know, my patient had only been so employed for about ten days. It would appear possible, therefore, that reversal of the usual habits, and, presumably, of metabolic rhythm, for a period as short as ten days may be sufficient to cause temperature inversion during the illness of a night worker. An occasional diurnal temperature higher than the nocturnal temperature might be explained by an error in the taking of the temperature. On some occasions false readings have been due to a diurnal atmospheric temperature transiently higher than that of the patient. These factors, however, are only likely to cause confusion on isolated occasions.

Temperature inversion is known to occur in pulmonary tuberculosis, and is said to occur also in typhoid and meningococcal infections. I. Herovici and W. Grünberg (*Bull. Soc. de Péd. de Paris*, 1932, xxx, 601) record several cases of inverse temperature in infants and in one adult, in all of which pyrexia was due to adenoid infection. The age, actual temperatures, and the occupation of the adult case are not recorded.

My patient, and those referred to by Dr. Lacey, were all night workers, and it seems that in lobar pneumonia the occupational factor may be the cause of the inversion. It would be interesting to hear the answer to the question asked in 1911 by Dr. Wm. Downing—namely, whether practitioners in attendance on men whose employment takes them on night duty have observed similar phenomena, not only in pneumonia, but in other pyrexial illnesses.—I am, etc.,

London, N.13, March 16th. C. ALLAN BIRCH.

Chronic Respiratory Disorders and the Autonomic System

SIR,—To the careful observer it may appear that the significance of the autonomic nervous system in relation to respiratory disorders has not yet been fully recognized. Important data which exist remain uncorrelated, awaiting admission as it were at the door of the respiratory diseases bureau, certain sections of which are replete with the speculative philosophy of eighteenth and nineteenth century teaching. The relationship of the heart's action—in order and disorder—to the autonomic nervous system has been explored, and the teaching of cardiac ailments placed on a sound anatomical and physiological basis of fact. The treatment of heart disease is now founded on exact knowledge. As one who has studied chronic diseases of the chest from the anatomical point of view during the last decade and longer at a well-known chest hospital of the metropolis, where diagnosis and treatment of chronic respiratory diseases form one of the principal items of business, may I venture to say that the same happy revolution awaits the treatment of certain chronic respiratory disorders when their relation to each other and to the autonomic nervous system is brought to light and acknowledged, as took place when cardiology was scrutinized in the light of anatomy and physiology.

The autonomic nervous system controls the activity of the respiratory tract, and it is the only source of nerve supply to lungs, bronchi, and trachea. Does this complicated nervous organization—with its vagal and sympathetic components distinct in structure, antagonistic in action, the essence of whose activities is co-ordination—never function amiss? If the inquirer seeks for an answer in the textbooks he will find very little information. In certain groups of respiratory disorders the teaching appears to be of the nature of medical traditionalism reverently handed down to succeeding generations, and accepted now as gospel. Is it not time to cease the classification of

respiratory diseases according to topographical areas—"diseases of the upper respiratory tract," "conditions affecting the air in the alveoli"? May we not question the wisdom of placing asthma in an isolated compartment as if it had no possible relations among other respiratory disorders?

This letter is not intended as a sweeping criticism of present-day teaching of respiratory disorders in general—very much the reverse. Notable progress has been made recently in the diagnosis and treatment of tuberculous abscess, bronchiectasis, etc. It has been brought about by the penetration of the anatomical and surgical spirit into the mediaeval atmosphere of the chest hospital; and this was not always due to the surgeon—more often the credit has seemed to belong to the enlightened physician with the surgical outlook. In a recent letter in the *British Medical Journal*, the writer, praising an article by an acknowledged authority on the subject of chronic bronchitis, says: "The indications for medical treatment are much the same as were given twenty-seven years ago." With truth he might have written: "There is little new in the diagnosis and treatment of chronic bronchitis since Laennec published his classical description of it and bronchiectasis nearly one hundred and twenty years ago." Nothing new except perhaps the bacterial heresy and its implication—introduced in the eighteen-eighties—when bacteriology was appropriating every disease in medicine, chronic bronchitis included, which was not secured on a firm anatomical foundation.

If there were not something fundamentally amiss in the present-day conception of chronic bronchitis, asthma, bronchiectasis, and emphysema, the treatment of these disorders would not be contemplated with feelings of despair by the present-day practitioner. The truth, as it appears to me, is that these four conditions might fittingly be placed together in a section in textbooks entitled "Disorders of the Autonomic Nervous System," asthma and chronic bronchitis being disorders of function, emphysema and bronchiectasis being resultant associated anatomical conditions. What is the present stagnant attitude towards these four respiratory diseases due to? I suggest three causes. (1) Clinging to the stethoscope and relying on it as the adequate, sole, and sufficient instrument for diagnosis. (2) The too infrequent use made of radio-opaque substances and the *x* rays for investigation (and also for treatment); the bronchoscope might more frequently be employed with advantage. (3) The empirical and arbitrary classification of respiratory disorders by topographical areas instead of studying anatomical relationships for the purposes of grouping.—I am, etc.,

London, W.1, March 19th. J. B. CHRISTOPHERSON, M.D.

A Common Final Examination

SIR,—In his letter published by you on January 26th (p. 182) "A Layman" says that a legal point in the Charters of one of the bodies concerned blocked the scheme for establishing a common final examination for admission to the *Medical Register*. As I am interested in everything that has to do with medical licensing and teaching, I should be glad to know something further of this project that went astray.—I am, etc.,

Bristol, March 12th. J. A. NIXON.

*** We have referred this inquiry to the writer of the letter, whose reply is as follows.

SIR,—The only information I have on the point is to the effect that in 1872 a scheme for creating an Examining Board in England under Section XIX of the Medical Act of 1858 was prepared and approved by the two Royal Colleges, the Society of Apothecaries of London, and the Universities of Oxford, Cambridge, Durham, and London. At the last moment it was discovered that the Charters of the Society of Apothecaries precluded association with other licensing bodies, and the scheme, in consequence, was dropped.

In 1875 Sir James Paget, who was then President of the Royal College of Surgeons of England, addressed a letter to each of the universities and medical corporations in England calling a conference of representatives of the licensing bodies "engaged on the formation of a scheme for an Examining Board in England" for the purpose of amending and recasting the scheme of 1872. By this date the Society of Apothecaries had amended its Charters, and was able to become one of the co-operating bodies. This scheme was completed in 1877, and received the blessing of the General Medical Council. Exactly why it was never acted upon I have not yet ascertained.—I am, etc.,

March 15th. A LAYMAN.

Post-mortem Examinations

SIR,—I write to commend the letter from Dr. F. E. Loewy in the *Journal* of March 16th (p. 563). It is to be hoped that the point he raises will not be allowed to become submerged again.

To my mind one of the chief drawbacks of general practice, particularly country practice, is the lack of any way of learning from one's mistakes, or indeed of knowing very often which are mistakes. Moreover, much valuable pathological material is daily being abandoned to our cemeteries; and some service such as your correspondent suggested might do something to prevent this waste. It would not, of course, entirely solve the difficulty. For instance, recently I was willing to go to considerable trouble to acquire the spinal cord of a man who died of a cerebral haemorrhage when showing a typical crop of shingles, but prejudice defeated me. Again, I heard recently of a case where a probable compensation of some hundreds of pounds was forgone by relatives rather than allow a post-mortem examination.—I am, etc.,

Redruth, Cornwall, March 22nd. L. A. RIDDELL.

Advertised Cures for Cancer

SIR,—The following advertisement appeared in the Glasgow *Citizen* of March 18th, 1935 (I omit the advertiser's name, address, and telephone number).

> **CANCER**
> **KNOCK-OUT FOR CANCER**
>
> Glasgow Gentleman offers his services to sufferers from this so-called dreadful Disease. It is the writer's opinion this disease is not so deadly as it is said to be, and he claims to destroy the cancer germ by Nature's methods. No Drugs. These are not merely words but facts which will prove a blessing to such sufferers.
>
> Heart, Stomach and Liver complaints successfully treated.
>
> Consultations by Appointment
>
> Serious cases treated at their own homes.

No doubt the Press normally exercises some discretion in the publication of advertisements of this type, but it is clear that loopholes exist and that advantage is taken of them. Would it be possible for the General Medical Council to instigate legislation to prevent this sort of thing, and to make it a punishable offence for unqualified persons to advertise their ability to cure disease? The present example makes one long for the methods of the "Four Just Men," but the penalties for taking the law

into one's own hands are severe, and I can only hope that our legislators may find time to protect cancer sufferers, perhaps the most pathetic of all our fellow citizens, from those who are ever ready to relieve them of their money, if not of their disease.—I am, etc.,

P. R. PEACOCK,
Director, Research Department, Glasgow Royal Cancer Hospital.

Glasgow, March 19th.

"Art and Fashion in Medicine"

SIR,—The fact that the Hastings Lecturer repeats, in spite of protests, his criticisms of homoeopathy makes a "protestant" wonder if Sir Walter Langdon-Brown's own conceptions of Hahnemann have not crystallized into a complex.

The modern homoeopath is no sectarian and dislikes the need of a label. But there is a danger that the teachings of Hahnemann, many of which are of the utmost value, may become lost to the profession. Such a grave misfortune has so far been prevented by the care of a body of men who have inherited a label which is the relic of a bygone controversy. Hahnemann, to employ Sir Walter's simile, was not an engineer. He was interested in making driving as safe as possible; he drew up a highway code which still holds good for safety on the roads. For the human body has not evolved in perfection at the same rate as the motor car, and in spite of advances in biochemistry, bacteriology, and psychology, its reactions to disease remain the same as they were 150 years ago.

Hahnemann found that *similia similibus curantur* was a useful clinical generalization, and his experience has been confirmed by thousands of the profession since 1790. The biological explanations of this generalization are yet to be discovered, but "engineers" such as Boyd of Glasgow, Martiny of Paris, and Perrson of Leningrad are working at this problem. "We are free to cast these [ideas] aside," claims the Regius Professor. I hope for the sake of the future of medicine that this is to be interpreted as a royal "we," and that the Hastings Lecturer does not assume the responsibility of rejecting Hahnemann's ideas on behalf of the whole profession. —I am, etc.,

FRANK BODMAN, M.D.

Bristol, March 20th.

SIR,—We cannot expect the homoeopaths to forgo calling all others "allopaths." It is plausible, and very valuable as propaganda. Politicians have long known how a judiciously chosen label for those not of their colour creates a useful prejudice and gains many adherents who could not be gained by reasoning. Imperialists spoke of Little Englanders, the Communist speaks of the Bourgeoisie. The homoeopath's linguistic habit is probably incurable, unless it yields to treatment with similars. Are they faith-healers? If not, by their own type of reasoning, they are faithless healers. They are either nature curers or unnatural curers, devotees of new thought or else obsolete thinkers; heliotherapists or lovers of darkness; chiropractors or advocates of scoliosis.—I am, etc.,

S. P. CASTELL.

London, N.7, March 23rd.

Obituary

T. JEFFERSON FAULDER, F.R.C.S.

Senior Surgeon to the Golden Square Throat Hospital

We regret to announce the death, on March 20th, of Mr. T. Jefferson Faulder at the age of 63, after a short illness. He had been surgeon to the Golden Square Hospital for Diseases of the Throat, Nose and Ear for the past twenty-seven years.

Thomas Jefferson Faulder was born at Carlisle in 1871, and received his early education at St. Bees School, Cumberland. He was at Clare College, Cambridge, from 1889 to 1893, held the Grindal exhibition and the senior classical scholarship, and obtained a first class in the classical tripos in 1893. For some years subsequently he was engaged in teaching, but later he entered St. Bartholomew's Hospital as a medical student, and proceeded to add to his already distinguished academic record by winning the Jeaffreson exhibition in 1898; the first scholarship in chemistry, physiology, and histology in 1899, in anatomy and biology in 1900; and the Brackenbury scholarship in 1903. In this last year, also, he obtained the diplomas M.R.C.S., L.R.C.P. He became a Fellow of the Royal College of Surgeons in 1904, and graduated M.B., B.Ch. at Cambridge in the following year. He was a research scholar at the London School of Economics in 1907.

To one of his outstanding ability appointments came quickly and deservedly at St. Bartholomew's Hospital. In 1904 he was house-surgeon to Mr. Harrison Cripps. He was assistant demonstrator of anatomy from 1905 to 1908, and chief assistant in the throat department from 1907 to 1913. His interest in throat surgery was steadily developing, but he found time for general surgery at the St. Marylebone Dispensary, although the laryngological work at St. Bartholomew's and the Golden Square Hospital was always his first joy. Then came the war and a long and exhausting period of service at the front. For many years Faulder had taken interest in the Territorial Force, and he served as major in the R.A.M.C.(T.) with the 92nd Brigade R.F.A. and the 2nd London Division. He was awarded the 1914–15 Star, and the Territorial Decoration later. He wrote two articles, drawn from his war experiences, one in the *Journal of Laryngology*, in 1921, on injuries to the ear in modern warfare, and the second in the *British Medical Journal*, in 1925, on occupational diseases (military). After the war he returned to his consultant work in London and his hospital appointments. He was a Fellow of the Royal Society of Medicine, of the Hunterian Society, and of the West London Medico-Chirurgical Society. He was twice married, his first wife being the daughter of Mr. Harrison Cripps. Fishing and shooting were his two chief hobbies. He had been a member of the British Medical Association since 1909.

THE LATE DR. W. J. ADIE

Dr. Ruby O. Stern sends the following tribute. No account of W. J. Adie's work would be complete without some reference to his clinical teaching, for in this he excelled. Both at Charing Cross and at "The National" his out-patient clinics were invariably crowded to such an extent that late-comers stood no chance of hearing or seeing anything. The secret of his success as a clinical teacher lay in his method of approaching his patients. No patient was ever "a case," but essentially a human being whose illness was a problem which he had to solve. He really enjoyed out-patient work, and the short but comprehensive clinical lectures he gave on each patient were invaluable to those who had the privilege of listening to them. Even after many years of practice he took his patients' misfortunes to heart and grieved for their distress. It affected him deeply to have to give a bad prognosis. Adie was indeed "the beloved physician" to his patients. His students mourn a great teacher and cherish their memories of him.

JOHN RUHRÄH of Baltimore died on March 10th of this year. To many of the profession in this country his name is probably unknown, but to those who had the privilege of his friendship—and they were not only doctors—it means the loss of one who had, to an unusual degree, the gifts which bridge over the limits of nationality and distance. In the U.S.A. he was well known as a paediatrician before he had begun the studies which made him known in this country—the writings on the history of medicine, especially on the early literature of paediatric study. Some years ago he had written, with Dr. Mayer, a book dealing with poliomyelitis, and then in the summer of 1933, while he was in Naples, he himself was severely stricken with the disease. He struggled back to a moderate degree of physical activity, and though crippled and often in considerable pain he remained entirely undefeated in mind and resolution. His example and his insistence on the brighter aspects of his existence made him an inspiration to his fellow sufferers, and encouraged his friends on this side to the hope that he might once more visit England. He had intended to make the effort this year, and he would have had a warm welcome from all those who had once felt the charm and exhilaration of spirit which his personality always exercised. This brief note is a tribute from one who has known Dr. Ruhräh for a quarter of a century, and though meeting him but rarely in the last few years has always had him for a constant and valued correspondent who wrote (as he talked) wisely, wittily, and as if he had but just left your side and was to see you again to-morrow.

We regret to learn of the death of Dr. JAMES CRAN, O.B.E., V.D., at Belize, British Honduras, on February 8th. Dr. Cran was a member of the British Honduras Branch of the British Medical Association since its inception, and was generally recognized by his fellow practitioners as the doyen of the profession in that Colony. He studied medicine at Aberdeen, and graduated M.B., C.M. in 1895, proceeding M.D. in 1904. He went to British Honduras forty years ago and speedily built up a large practice, and actively identified himself with its social and political life. He served at varying periods of his career on the Legislative and Executive Councils, and on the Belize Town Board; he also acted on several occasions as P.M.O. for the Colony. He was instrumental in building up the British Honduras Defence Force, which sent a large quota of men to the front during the Great War. For his services he was given the rank of lieutenant-colonel and decorated with the O.B.E. Dr. Cran was a man of great personal charm, and was always deeply interested in the welfare of the Colony. The hurricane of 1931 was a disaster for him. Many of his old friends were killed, and the town and country he laboured in all his active life were laid in ruins. It was a terrible blow, from which he never seemed to recover. The medical profession and the community at large have expressed to his widow and surviving children their sincerest sympathy in the great loss they and the Colony have sustained.

By the sudden death of Dr. H. W. PARNIS, on March 8th, the London County Council Mental Hospitals service loses a valued medical officer. Henry William Parnis, the eldest son of Judge A. Parnis, was born in Malta on April 13th, 1889. He took the degree of B.Sc. of Malta University in 1907 and the M.D. in 1910. He then came to England, and in 1913 qualified M.R.C.S.Eng., L.R.C.P.Lond. The house appointments he held were at the Prince of Wales's General Hospital, Tottenham, the Royal Hospital, Richmond, and the London Temperance Hospital. In March, 1914, he was elected to the London County Mental Hospitals service and posted at Colney Hatch, but being seconded the following September he joined the R.A.M.C., and went to France that month with the 7th Division as medical officer to the South Staffs Regiment. He served in the Army, being mentioned in dispatches, until he was demobilized in July, 1919; nearly the whole of his war service was over-seas as regimental medical officer. On demobilization Dr. Parnis returned to his post at Colney Hatch Mental Hospital and in 1920 gained the D.P.M. Later he was promoted in the service and went to Claybury Mental Hospital. At the time of his death he was deputy medical superintendent at Bexley Mental Hospital, a post he had held since 1929. In 1931 he was appointed consultant in mental diseases to St. Alfege's Hospital, Greenwich. Dr. Parnis was a very able clinician and a most conscientious and painstaking medical officer. He took the greatest interest in all the activities of the hospital and in the welfare of his patients; indeed, his patients looked upon him not only as a physician but as a guide, philosopher, and friend. He will be sadly missed by a large circle of friends.

The death of Dr. E. M. FANNIN, Sliema, Sutton, County Dublin, which occurred on March 8th at a nursing home in the city, has caused deep regret among his many friends and patients. Dr. Fannin graduated B.A., M.B., B.Ch. of Dublin University in 1897, and was for many years attached to the Drumcondra Hospital. He was a Fellow of the Royal Academy of Medicine in Ireland, and at one time took an active interest in its administration. It was Dr. Fannin's grandfather who founded the old-established firm of Fannin and Company Ltd., Grafton Street, Dublin, which does the largest business in Ireland in medical and surgical supplies and an extensive trade in medical books. Dr. Fannin took a personal interest in the working of the Grafton Street premises, and contributed in its medical aspects to its general usefulness to the medical profession. A member of the Dublin Rotary Club, he attended its weekly meetings regularly, and helped a good deal in the success of its philanthropic undertakings on behalf of children.

The news of the death, on March 20th, of REGINALD JANSON HANBURY, M.R.C.S., L.R.C.P., of Foxbury, Woldingham, Surrey, will come as a shock to many friends in the medical profession, and to a wide circle of those engaged in the drug and chemical industry. Mr. Hanbury, who was in his fifty-eighth year, was the second son of Mr. Frederick J. Hanbury, the present chairman of Allen and Hanburys Limited. He was educated at Marlborough, and on leaving school became a student at St. Bartholomew's Hospital, where, in 1903, he obtained the English conjoint diplomas. Later he joined the business of Allen and Hanburys Limited, of which his grandfather, Cornelius Hanbury, was then chairman, and in 1912 was elected to the board. Mr. Hanbury was a governor of St. Bartholomew's Hospital, for several years serving on its Finance Committee, and had been a member of the British Medical Association for thirty years. He was a man of great personal charm, which won for him the affection and respect of all who came into contact with him, both in business and in private life. He leaves a widow and three children.

JAMES ROBERTSON REID, M.D., whose death has occurred at Newby, Scarborough, was born in Aberdeen in 1859, and studied medicine at Aberdeen University. While a student Dr. Reid joined the Aberdeenshire Royal Engineers, becoming a sergeant. He was present with this unit at Queen Victoria's review of the Scottish Volunteers at Edinburgh in 1881. After graduating M.B., C.M. in 1883 he practised at Helmsley, Yorkshire, for fifteen years before leaving for Southport, where he was in practice for twenty-five years. He served in the King's Liverpool Regiment, retiring with the rank of captain and honorary major. On the formation of the Territorial Force he received a commission in the R.A.M.C., in which he served until retired by the age limit. He held the Territorial Decoration. Dr. Reid was elected honorary associate of the Grand Priory of the Order of the Hospital of St. John of Jerusalem in England in 1909. He was the author of the recently published *History of the Clan Robertson*.

Medical Notes in Parliament

[FROM OUR PARLIAMENTARY CORRESPONDENT]

In the House of Commons during the past week the Government of India Bill was further considered in Committee. The Army, Air Force (Annual), and Consolidated Fund (No. 2) Bills were read a first time.

On March 20th, in the House of Lords, the Parliament Act, 1911 (Amendment), Bill was read the first time, and on March 21st the Notification of Poverty Bill was read the first time.

In the House of Lords on March 26th the Regimental Charitable Funds Bill was considered on report. The Increase of Rent and Mortgage Interest (Restriction) Bill passed through Committee. The Criminal Lunatics (Scotland) Bill also passed through Committee.

Brigadier-General Spears presented a Bill in the House of Commons on March 26th to Amend the Vagrancy Act, 1824, so far as it related to persons wandering abroad and lodging in barns and other places. The Bill ensures that no person shall be sent to gaol for sleeping out because he has not got the money to buy a night's lodging. On March 26th Mr. Runciman announced that he was setting up a formal investigation under the Merchant Shipping Acts into the loss of ships at sea. Lord Merrivale would preside over the inquiry.

Maternal Mortality

In the House of Commons on March 25th a debate took place on the motion for the second reading of the Consolidated Fund (No. 2) Bill. Mr. RHYS DAVIES, having expressed regret at the absence through indisposition of the Minister of Health, drew attention to the question of infantile mortality. He said that there had been a steady decrease in the rate during the last decade or so, and it was a remarkable achievement that they had been able to reduce the rate by one-half from 1905 to 1934. In 1905 the number of children dying before they reached the age of 12 months was 128 per 1,000 births; last year it was only 64 per 1,000. They would not, however, be satisfied to see a steady decline in well-to-do areas and a proportionate increase in the depressed areas. He was a member of the Joint Council of Midwifery, which came to a unanimous conclusion and put forward one of the most remarkable suggestions for reform that they had had for some time past. The report had been sent to the Ministry of Health, and he asked what was the attitude of the Ministry towards its recommendations. In 1923 the maternal mortality rate in this country was 3.82 per 1,000 live births, and in 1933 4.51—an increase of 18.46 per cent. in ten years. The total maternal mortality figures, including deaths from associated causes, were more menacing still. They were 4.83 in 1923 and 5.94 in 1933, or a rise of 22 per cent. The stillbirth rate rose from 38 per 1,000 in 1927 to 41 in 1933. What was the Government doing in the matter? Had the attention of the municipalities been called to the report of the Joint Council on Midwifery? There were 52,872 midwives on the roll in this country, but only about 15,000 of them were in serious practice. The council found that there were some women over 80 years of age practising as midwives. Until the training and competency of the midwives was improved there was very little hope for any decline in the maternal mortality rate. The proposal now made, among others, was that there should be established a municipal and salaried midwifery service.

Doctors and Midwifery

Mrs. TATE said that if our midwifery service was to be perfected we must limit training to those who intended to practise. A midwifery training should in no case be under three years. That would entirely exclude the general nurse who thought that in order to get a job she could take up her Central Midwives Board diploma and get it in six months. It was not enough to deal with the subject of midwives. At present the training of doctors where midwifery was concerned was grossly insufficient. The report on maternal mortality showed that instead of the twenty cases—which was little enough—which a student was required to study before supervising the bringing of babies into the world, in a great many instances "only as many as 2.8 cases had been the actual number which students had studied." The whole question of medical knowledge of midwifery should be specialized. Sir ARNOLD WILSON said that the obstetric service provided by doctors in this country was now recognized by the medical profession as inadequate, and it could not be dealt with merely by improving the training of doctors. The problem was one of hospital management and administration, and should be dealt with by the Ministry of Health as a whole. One factor which was probably at the bottom of the increase in maternal mortality in the last three or four years was puerperal sepsis consequent on unskilled abortion. There were scores of thousands of women who were permanently disabled, not from accidents after birth, but from abortions, involuntary or deliberate, under unhealthy and undesirable conditions. It was time we recognized and took stock of that fact. One of the first essentials in the care of women before and after childbirth should be attention to the condition of their teeth, which had been damaged by lack of calcium. At present less than one-third of all the women under the national health insurance scheme were entitled to dental treatment under that scheme. Speaking as chairman of the executive committee of the Ivory Cross, he submitted that no form of expenditure would be better justified by immediate results than an increase in the dental benefit services, and some system whereby those services would become universal.

A Medical Reply

Dr. O'DONOVAN said that there was need, in discussing the question of obstetrics, of a certain care in the use of words. For members of the House to talk about admissions of failure in the care of parturient women might have ill effects which the speakers might have hardly considered. The report of debates in the Press were read by many young pregnant women, whose nervous tone had not always the stability of the toughened men and women politicians. There was no question that if there was too much and too morbid discussion in public assemblies with regard to the inescapable risks of pregnancy, there would grow up a widespread anti-conception practice such as was never dreamed of when Ignaz Phillip Semmelweis discovered the cause and prevention of contagious puerperal sepsis about 1850. Since he was a student he had read many reports about the morbidity of pregnant women, and it was sometimes a source of astonishment to him that practitioners had stood up so manfully to their work. After having been shrewishly scolded and condemned as being untrained and incompetent, they had stuck night and day to their obligation of assisting women in the most difficult period of their lives. That day he had noticed with pleasure a welcome change in the tone of the discussion. The doctors were no longer *particeps criminis*, but it was strange that so many lay interveners had said that the doctors' knowledge was incomplete, that they needed more research and more training. Describing the conditions under which students were trained in obstetrics, Dr. O'Donovan said that the responsibility in the end was not that of Whitehall officials or the Government chiefs; it was inescapably the responsibility of the medical profession. The present Government and the London County Council had given generous assistance to the establishment of the post-graduate school of medical studies at Hammersmith, and if there was any demand for gynaecological clinics it was to be hoped that the Government would hold its hand and let the medical profession first produce competent gynaecologists before gynaecological clinics were scattered all over the country. New buildings, devoted nurses, and equipment did not make competent doctors, and unless they had first-class gynaecologists to start with, gynaecological clinics would be little more than disinfected first-aid stations. The College of Obstetricians had received a charter since the war in order that those who taught this special subject might be able to meet and discuss such problems, and give diplomas of special competency to those who would be assisting in future to man the establishment of gynaecological clinics. At the Royal Society of Medicine all the teachers of and specialists in

obstetrics met regularly, and such knowledge and experience as they had acquired were interchanged with the utmost freedom. There was no monopoly of obstetric knowledge in London; it was distributed from there to teaching centres throughout the Empire, and was reciprocally received. Then again, the British Medical Association held Annual Meetings, at which it invited the leading authorities and those who had done notable research in the last few years to address the whole profession on the results of their work. Not content with that, it had a system by which it sent specialists in the work up and down the country to lecture at professional meetings, so that what advance was made in the teaching centres was rapidly brought into the homes of the general practitioners and into the bedrooms of their patients.

Government's Special Inquiries

After further debate, Mr. SHAKESPEARE said that the question of maternal mortality was one which needed constant care and supervision. The Government, in dealing with the prob'em, was trying a general attack over the whole country. In October a circular was issued asking all maternity and child welfare authorities to survey urgently their services. So far the result had been satisfactory. Of the 421 authorities, 155 had replied that they were taking steps to improve or to extend their services; 174 had reported that they did not propose improvements, but in only twenty-three of those cases were the services regarded as not reasonably complete; and eighty-four promised attention. He need hardly say that the Ministry was in touch with every authority which admitted that improvement was necessary, either by correspondence, by interviews with the department, or by inspectors on the spot. In the circular a special attack was directed on the black areas, where the figures showed that for some reason or other there was an abnormally high maternal mortality rate. It was clear that if they could find out the reasons in those areas, and take effective steps to remedy them, the percentage would appreciably fall. The black areas consisted of fourteen counties and twenty-two county boroughs. The medical officers of health had been asked to make an urgent report to the Ministry covering these points: what improvement of the ante-natal service was necessary; the measures taken in the district for the provision of a midwifery service for all confinements, and the provision of adequate beds for complicated cases in hospitals; the steps taken for the isolation of cases of puerperal sepsis; and arrangements for consultant services in difficult cases. When they received those reports they would have the advantage of the services of an eminent obstetrician, not of the Ministry of Health. They sent down to each of these areas two inspectors from the Ministry of Health, one a medical officer with an administrative knowledge of this question, and the other a lady doctor with a long experience of the maternity side. These two, together with the medical officer on the spot, went in to the report and found out where were the weak places in the provision of these services. They had already dealt in this way with Blackpool, Wigan, Plymouth, and Wakefield, and they always had a check borough so that they could make comparisons. Visits were in progress in the county boroughs of Preston, Barnsley, and Dewsbury. As regarded South Wales, where the black spots were in two counties, special inquiries were being made in Carmarthenshire and Denbighshire, and an inquiry into Monmouthshire was in progress. Towards the end of the year they hoped to consolidate all these reports and to issue a report.

Joint Council of Midwifery's Report

The report of the Joint Council of Midwifery was a most valuable document, and the mere composition of the council meant that any Government must of necessity give it the most careful consideration. As soon as they received it the department got into touch with the representative associations of local authorities, the County Councils Association, the Association of Municipal Corporations, and the London County Council, and asked them to furnish their views. Meanwhile in the department they were giving it special study. The training of medical students had been considered by the General Medical Council, and recently the curriculum had been strengthened to see that there was a more extensive course of training and examination given to students in the question of midwifery. He understood that the Central Council of Midwives was considering the same thing, and he expected that shortly an agreement would be reached by which the course of training of midwives would be extended and improved. With regard to the relation of malnutrition to maternal mortality, common sense would dictate that if a woman was ill nourished she would be less able to bear the shock of childbirth than the woman who was well nourished. The medical evidence that they had, strangely enough, did not support the common-sense point of view. Indeed, the most expert survey undertaken by the departmental committee in 1932, which examined in great detail a large number of cases, came to the conclusion that at least half the cases of maternal mortality occurred where there was no poverty at all. That was one of the things which baffled all investigation, and made it quite impossible to deal with this as a matter of poverty.

On March 21st Sir F. FREMANTLE asked the Minister of Health if he had seen the official statement by Mr. Hughes, Federal Minister of Health of Australia, that the Australian maternal mortality rate was 5.57 per 1,000 and that the English rate, if compiled on the same basis as the Australian, would be higher; and whether he would give the figures for this country compiled on the same basis as the Australian figure above mentioned. Mr. SHAKESPEARE said that the Minister had seen a report in the Press to the effect of the first part of the question, but the statement as to comparative rates in the two countries was not attributed to the Federal Minister of Health. There were known to be certain differences in the basis of calculation in the two countries, and the Minister doubted whether it was possible to establish the true relation between the English and Australian rates without much fuller comparison than had hitherto been made between the English and Australian procedure at all stages in the preparation of maternal mortality statistics. Sir F. FREMANTLE asked if nothing was being done to get a system by which we could compare these figures with different parts of the Empire. Mr. SHAKESPEARE: Not so far as I know. Sir F. FREMANTLE: Will my hon. friend look into the matter with the Registrar-General and see if such a very obviously elementary procedure could not be adopted?

Medical Referee in Yorkshire

On March 21st Mr. T. WILLIAMS asked the Home Secretary if he was aware of the feeling in Yorkshire against the appointment of Dr. Caiger as medical referee in eye and nystagmus cases, owing to the fact that the same doctor generally acted on behalf of the Coal Owners' Indemnity Company in arbitration cases, and if he would endeavour to appoint one or more referees who refrained from acting either for employers or employees. Sir JOHN GILMOUR said that he had not received any representations from the miners or other workmen against the appointment of Dr. Caiger, who had been a medical referee for nearly fifteen years. He assumed that Mr. Williams's question was not intended in any way to imply that Dr. Caiger had discharged his duties as referee otherwise than impartially, but that it had been put with the object of drawing attention to the desirability of appointing as medical referees doctors who were not called on to deal with such cases in their private practice. As he had indicated in replies to questions by the hon. member a week ago, he was certainly prepared to consider the problem, but he must again emphasize that it was one which presented great practical difficulties. Mr. WILLIAMS asked if the Home Secretary's Department was aware of the deadlock which had been reached in Yorkshire, since the doctor referred to in the question had already examined a person three times on behalf of the indemnity company, and that there were no other medical referees available who had not taken part on the coalowners' side. He asked the Home Secretary, during his investigations into the possibility of appointing a board of medical referees, to consider the advisability of appointing as referees in Yorkshire persons who had not served either the employers or the employees. Sir JOHN GILMOUR replied that he was very conscious of the necessity of trying to reach a fair decision in these matters. The difficulty was to find men with technical knowledge of eye problems. He was ready to look into the problem, and he would be glad to confer with Mr. Williams on the subject.

Scarlet Fever Outbreak at Denham

On March 21st Mr. SHAKESPEARE informed Mr. Thorne that the Minister of Health had received a report from his medical officer on the scarlet fever outbreak at Denham, Bucks. The number of children affected was eighty-one. Only the two elementary schools affected had been closed on account of the outbreak. The outbreak was found to be due to the consumption of raw milk, which had been neither pasteurized nor boiled, and the medical officer of health at once arranged for the pasteurization of all further milk from the dairy concerned. This precaution effected the immediate arrest of the outbreak. The source of the infection was later traced to a milker on a farm supplying the dairy, and he was removed to the isolation hospital for observation. The Minister of Health was advised that adequate precautions had been taken to prevent any further spread of the infection.

Coroners and Post-mortem Examinations

On March 26th Sir JOHN GILMOUR told Mr. Groves that his attention had been called to the dissatisfaction existing in certain areas, especially in East Ham, concerning the policy of the coroner in employing for all post-mortem examinations one particular pathologist. Since the passing of the Coroners (Amendment) Act, 1926, it had been open to a coroner to request any legally qualified medical practitioner to make a post-mortem examination, and he had no power to interfere in any way with the discretion which was conferred on the coroner by statute.

The Hallam Hospital, Bromwich.—On March 18th Mr. SHAKESPEARE informed Mr. Lovat-Fraser that he was aware that the Staffordshire Public Assistance Committee had recently issued instructions that patients in the Aldridge urban district area should be accommodated at the Hallam Hospital, Bromwich, instead of at the Manor Hospital, Walsall, as heretofore. The Minister of Health was not responsible for this decision. He had received a protest from the urban district council and was in communication on the matter with the county council, with which the responsibility in the matter rested.

Deaths after Vaccination.—Sir HILTON YOUNG, on March 18th, informed Mr. Groves that seven death certificates were received during 1934 in which death was attributed to vaccinia or vaccination, or was in some way associated with vaccination. The ages of the persons concerned were 3 months, 8 months, 6 years, 7 years, 18 years, and 20 years respectively.

Diphtheria Immunization.—On March 18th Sir HILTON YOUNG informed Mr. Groves that, according to information furnished by the local authority, 3,631 children at Acton had received three doses of diphtheria immunizing mixture and 2,063 of these were re-Schick-tested subsequently. Of the latter number 2,050 tests gave negative results. The figures were in each case brought up to December 31st, 1934.

Maternal Mortality Report for Scotland.—On March 19th Sir GEOFFREY COLLINS informed Mr. Guy that every effort was being made to expedite publication of the report of the Scientific Advisory Committee of the Scottish Department of Health on maternal mortality and morbidity. The inquiries had involved the scrutiny of over 42,000 cases.

Housing Bill in Committee.—The Standing Committee of the House of Commons resumed consideration of the Housing Bill on March 19th. On Clause 33, which provides that local authorities in receipt of a State subsidy under the Bill shall make a contribution from the rates towards the expenses of rehousing, Sir F. FREMANTLE moved to amend the provision in the clause which empowers the Minister of Health to continue, reduce, or stop the subsidy on certain dates. The object of the amendment, he said, was to make allowance for circumstances in which the local authorities could not complete their houses within the specified time. Mr. SHAKESPEARE said that it would be a mistake to introduce elasticity into the provision. The clause enabled the Minister to review the State contribution on October 1st, 1937, and on each third succeeding year. If an element of doubt were introduced, and local authorities thought it did not matter whether they cleared their slums before the specified date, administration would be paralysed. There was a possibility that local authorities in many cases would be able to complete their task in less than the prescribed period of five years, so that there would be a considerable period of latitude allowed to them for circumstances beyond their control. Sir F. FREMANTLE in intimating that he would not press the amendment, said he did not think the difficulties of giving effect to it were so great as Mr. Shakespeare had indicated. The amendment was withdrawn, and the clause ordered to stand part of the Bill. The committee adjourned until Tuesday, March 26th.

Tubercle Bacilli in Milk.—Brigadier-General CLIFTON BROWN, on March 21st, asked the Minister of Health whether he was aware that the Cattle Diseases Committee reported in 1934 that the frequent infection by tubercle bacilli of milk sold from licensed and unlicensed pasteurization and other heat-treatment plants constituted a disturbing situation, and what, if any, steps had been taken since the issue of the report to draw the attention of medical officers to its contents. Mr. SHAKESPEARE referred to an answer given on this subject on March 14th. He added that the circular which the Minister of Health was sending to local authorities would draw attention to the report of the Cattle Diseases Committee, and copies of the circular would be sent to medical officers of health.

Ex-Service Patients.—On March 21st Major TRYON told Mr. Crooke that at the end of December last approximately 2,400 officers and men were receiving in-patient treatment, apart from patients in mental hospitals. Certainly the great majority of those receiving in-patient treatment would be in receipt of treatment allowances.

Bugs in Barracks.—Mr. D. H. HACKING, replying on March 21st to Sir Arnold Wilson, who asked if his attention had been called to the infestation of barracks in Egypt, Gibraltar, China, and Malta by bugs, said that perhaps the hon. gentleman would refer to the report on the Health of the Army for 1933, which showed the varying success that had attended experiments to rid infested barracks and quarters of this pest. The problem, however, was a very difficult one, and the only satisfactory solution lay in the replacement of hutted structures and the modernization of old-fashioned permanent buildings, which was being done as funds became available. At the present time extensive schemes were in contemplation in China and Egypt.

Unemployment Benefit for Women.—On March 25th Major HILLS asked the Minister of Health if an insured woman who was unable to follow her occupation on account of pregnancy, but was not certified under the National Health Insurance Acts as being incapable of work, was treated as eligible for unemployment benefit. Mr. R. S. HUDSON replied that each case was dealt with in the light of its circumstances, but he was not aware that there was any gap between national health and unemployment insurance in such cases.

Examination of Chinese Eggs.—Mr. SHAKESPEARE informed Mr. Haslam that samples of Chinese eggs had recently been examined in the laboratory of his department and nothing was found to show that any pathogenic organisms were present. Mr. HASLAM asked if the hon. member was aware that the examination showed that there were bacteria causing decomposition, and if he would ask the Minister of Health to publish a fuller report than he had indicated to the House, showing exactly the numbers of these micro-organisms, the rate of decomposition, and other matters of importance to public health. Mr. SHAKESPEARE replied that the Department of Health was asked to make the examination, and did so, and the result did not confirm Mr. Haslam's fears. Sir F. FREMANTLE asked if Mr. Shakespeare suggested that his department only condemned food when it had pathogenic organisms—that was, organisms which produced disease—and did not mind organisms which only produced decomposition. No reply was given to this question. Mr. SHAKESPEARE told Mr. Lambert that the report he had seen of the bacteriological examination recently carried out at the Devonport laboratory gave no indication of the food values of eggs imported from China.

Notes in Brief

Mr. HACKING states that the cost of modernizing existing barracks and hutments at home and abroad, excluding India, is estimated at approximately £12,000,000.

Medico-Legal

SETTLEMENT OF PHYSICIAN'S LIBEL ACTION

In the King's Bench Division, on March 22nd, before Mr. Justice Avory, it was announced that a settlement had been reached in an action in which Lord Horder claimed damages for libel from Odhams Press, Limited, owing to an article which appeared in *John Bull* in February, 1934. Mr. Norman Birkett, K.C., who appeared for Lord Horder, stated that the article, which was headed "The Noise that Kills," was represented to be a special interview granted by Lord Horder to that paper, whereas in fact Lord Horder had given no interview and had in no way authorized the publication of the article. The defendants had expressed their deep regret for having stated that the article was the report of a special interview. The editor had been misled by a contributor. The sincere apology of the defendants was tendered to Lord Horder, and, the plaintiff not desiring himself to profit from the action, they had paid a substantial sum to St. Bartholomew's Medical School, which had been nominated by him for the purpose.

The judge thereupon directed that the record be withdrawn.

Universities and Colleges

UNIVERSITY OF OXFORD

The Electors have notified to the Master and Fellows of University College the election, on March 13th, of R. C. MacKeith, B.M., to a Radcliffe Fellowship. At a meeting of the Master and Fellows of University College, on March 20th, the Radcliffe Prize for the furtherance of medical science in the University was awarded, on the report of the examiners, to C. L. Cope, D.M.

UNIVERSITY OF LONDON

A meeting of the Senate was held on March 20th, with the Vice-Chancellor (Professor L. N. G. Filon, D.Sc., F.R.S.) in the chair.

Dr. John Gray was appointed Reader in Morbid Anatomy at the British Post-Graduate Medical School as from April 1st.

It was resolved that, on his retirement from the professorship of surgery at St. Bartholomew's Hospital Medical College at the end of the present session, the title of Emeritus Professor in Surgery in the University should be conferred on Mr. G. E. Gask, C.M.G., D.S.O.

Sir A. Daniel Hall, K.C.B., LL.D., F.R.S., was appointed Heath Clark Lecturer for 1935 in place of Professor Karl Pearson, who is unable to deliver the lectures.

UNIVERSITY OF LIVERPOOL

The following candidates have been approved at the examinations indicated:

DIPLOMA IN MEDICAL RADIOLOGY AND ELECTROLOGY.—*Part A:* F. O. Pilkington, R. G. Reid, E. Siung, R. F. Wynne.

D.P.H.—*Part I:* A. R. Arulpragasam, A. T. Jones, E. Somasekhar, H. E. C. Sutton, J. V. Walker.

DIPLOMA IN TROPICAL MEDICINE.—A. J. Board, Freda M. B. Harmer, A. Kamel, C. H. Li, J. R. Robertson, *H. R. Shone, S. L. Strange, J. W. C. Symonds, A. J. Walker.

DIPLOMA IN TROPICAL HYGIENE.—S. O. Awolivi, S. W. Cooper, P. C. Cosgrove, R. Y. Dunlop, K. C. Mackenzie, Z. Z. Nyi.

* Recommended for the Milne Medal.

UNIVERSITY OF BRISTOL

Miss Dorothy Woodman, M.D., M.Sc., has been appointed lecturer in pathology; Dr. H. J. Orr-Ewing, F.R.C.P., junior tutor in medicine; and Miss G. R. Borthwick, B.Sc., assistant bacteriologist.

UNIVERSITY OF EDINBURGH

At its meeting on March 19th the University Court approved the establishment of a Sharpey-Schafer Lectureship in Physiology, a fund for its endowment having been contributed by pupils and friends of Emeritus Professor Sir Edward Sharpey-Schafer, F.R.S.

It was agreed to join the other Scottish universities and other organizations in submitting evidence before the Select Committee of the House of Lords in opposition to the Osteopaths Registration Bill.

The Senatus Academicus has resolved to confer the honorary degree of LL.D., on June 28th, upon John Donald Pollock, M.D., C.M., honorary surgeon commander, R.N.V.R., and Alfred Newton Richards, M.D., Professor of Pharmacology in the University of Pennsylvania, Philadelphia.

UNIVERSITY OF GLASGOW

At the graduation ceremonial to be held on June 19th the honorary degree of LL.D. will be conferred upon Emeritus Professor Walter King Hunter, M.D., D.Sc., Professor J. M. Munro Kerr, M.D., and Dame Louise McIlroy, M.D., D.Sc., late Professor of Obstetrics and Gynaecology, London School of Medicine for Women.

The Services

COMMISSIONS IN THE R.A.M.C.

The War Office announces that applications are invited from medical men for appointment to commissions in the Royal Army Medical Corps. Candidates will be selected for commissions without competitive examination, and will be required to present themselves in London for interview and physical examination on or about April 25th, 1935. They must be registered under the Medical Acts, and normally must not be over the age of 28 years.

Successful candidates will in the first instance be given short service commissions for five years, at the end of which period they may either retire with a gratuity of £1,000 or apply for a permanent commission. Permanent commissions will be given to officers selected from among those who wish to make the Army their permanent career.

Full particulars of the conditions of service and emoluments, also forms of application, may be obtained on application, either by letter or in person, to the Assistant Director-General, Army Medical Services, The War Office, London, S.W.1.

NAVAL MEDICAL COMPASSIONATE FUND

A meeting of the subscribers of the Naval Medical Compassionate Fund will be held on April 26th, at 3 p.m., at the Medical Department of the Navy, Admiralty, S.W.1, to elect six directors of the Fund.

The King has conferred the Efficiency Decoration of the Territorial Army upon the following officers of the Royal Army Medical Corps (Territorial Army): Colonel G. L. Thornton, *M.C.* (ret. T.A.), Honorary Colonel, 43rd (Wessex) Division; Lieut.-Colonel and Brevet Colonel R. Errington, *M.C.*; Lieut.-Colonels E. L. Sandiland and S. J. C. Holden; Majors H. Shield, *M.C.*, and J. C. MacKay, *M.C.*; and Captain G. W. Wright, M.M.

Medical News

A discussion on "Maternal Mortality," to be opened by Mr. Eardley Holland and Dr. R. Veitch Clark, will be held at the Royal Sanitary Institute, 90, Buckingham Palace Road, S.W., on Tuesday, April 9th, at 5.15 p.m. The chair will be taken by Dr. George F. Buchan.

Sir John Gilmour, Secretary of State for the Home Department, will open the Metropolitan Police Laboratory for the Scientific Investigation of Crime, at the Metropolitan Police College, Hendon, on the afternoon of Wednesday, April 10th.

The annual meeting of the Society for the Study of Inebriety will be held at 11, Chandos Street, W., on Tuesday, April 9th, at 4 p.m., and will be followed by a discussion on "What is addiction?" to be opened by Dr. E. W. Adams.

The annual meeting of the Medical Officers of Schools Association will be held at 11, Chandos Street, W., on Friday, April 5th, at 5 p.m., when a discussion will take place on "Emotional Disturbance in Adolescence."

A post-graduate course in the medical and hydrological treatment of vascular, renal, rheumatic, and humoro-vegetative affections will be held at the Hôpital Necker, Paris, from April 1st to 13th, under the direction of Professor Maurice Villaret. Further information can be obtained from the École Pratique, Faculté de Médecine, Paris.

The Fellowship of Medicine (1, Wimpole Street, W.) announces afternoon courses on infants' diseases, at the Infants Hospital, and on ophthalmology, at the Royal Eye Hospital, from April 1st to 13th. A course in proctology will be held at St. Mark's Hospital from April 8th to 13th. A special M.R.C.P. demonstration of the fundus oculi will be given at the in-patient department of the West End Hospital for Nervous Diseases on April 2nd, at 8.30 p.m. Other forthcoming courses include psychological medicine, at Maudsley Hospital; dermatology, at St. John's Hospital; medicine, surgery, and gynaecology, at Royal Waterloo Hospital; thoracic surgery, at Brompton Hospital; and week-end courses in cardiology, at Victoria Park Hospital; chest diseases, at Brompton Hospital; and obstetrics, at City of London Maternity Hospital. A panel of clinical teachers is available. Detailed syllabuses may be had on application.

The ninth Congress of French-speaking Gynaecologists and Obstetricians will be held at Algiers under the presidency of M. Laffont, from April 13th to 17th, when the following subjects will be discussed: treatment of chronic cervicitis, introduced by MM. Labry of Lyons and Villay of Bordeaux; obstetrical assistance in France, Belgium, and Africa, introduced by MM. Cocq and Mercken of Brussels and MM. Laffont and Fulconis of Algiers; and the oedemas of pregnancy, introduced by MM. Levy-Solat of Paris and Gueissaz of Neuchâtel, Switzerland. Further information can be obtained from Dr. Fulconis, 2, Rue Ampère, Algiers.

The tenth International Congress of the History of Medicine will be held in Madrid from September 23rd to 29th, when the subjects for discussion will be: Arabian medicine in Spain; medicine in America during its discovery and colonization; and medical folk-lore in various civilized countries. The subscription is 75 pesetas each for members of the International Society of the History of Medicine and 100 pesetas for others. Those desiring to attend the congress are invited to communicate with Messrs. Cook and Son, Berkeley Street, W.1, who have prepared a special inclusive scheme.

The following German congresses will be held in April: psychotherapy, Bad Nauheim, 11th to 13th; genetics, Jena, 24th to 26th; Röntgen Society, Berlin, 28th to 30th.

The 59th Congress of the German Society of Surgery will be held in Berlin from April 24th to 27th, when the chief subjects for discussion will be post-traumatic embolism, introduced by Stich of Göttingen; haemophilia, with special reference to its significance as a hereditary disease, introduced by Schloessmann of Bochum; the technique of sterilization, introduced by Bauer of Breslau; and the present position of the prostate problem, with special reference to operative technique, introduced by Voelcker of Halle.

The first overt step towards the establishment of a "House of Science" in London has been taken by the Society of Chemical Industry, the Chemical Society, and the Institute of Chemistry. A draft agreement for co-operation in administration has already been drawn up, and appears in the current issue of *Chemistry and Industry* (the journal of the Society of Chemical Industry).

Medical practitioners seeking advice on contraceptive methods or requiring details about equipment of a birth control clinic are invited to get in touch with the secretary of the Society for the Provision of Birth Control Clinics, 153A, East Street, Walworth, S.E.17.

W. Heffer and Sons Ltd. of Cambridge announce for early publication a volume entitled *The Schoolboy: A Study of His Nutrition, Physical Development, and Health*, by Dr. G. E. Friend, with a foreword by Professor J. C. Drummond.

The issue of *Paris Médical* for March 16th is devoted to cancer.

The issue of *Surgery, Gynecology and Obstetrics* for February 15th contains the proceedings of the twenty-fourth Annual Clinical Congress of the American College of Surgery, held at Boston on October 15th, 1934.

The Chinese National Health Administration has issued a notice, for guidance of foreign medical practitioners, to the effect that they should obtain licences to practise from the Administration, and their bona fides must be guaranteed by their consular authorities; they should learn Chinese, and should apply to the Permanent Chinese Delegation of the League of Nations at Geneva before setting out for China.

A central office for German and international scientific congresses has been established at the Langenbeck-Virchow Haus, Luisenstrasse 58, Berlin.

A central cancer institute has been established in the Rudolf Virchow Hospital, Berlin, with Professor Sauerbruch as curator.

From September 2nd to October 20th, 1934, 2,401 definite cases of typhus and thirty-three suspected cases were notified in Chile, or a weekly average of about 350, as compared with a weekly average of over 500 in 1933.

Letters, Notes, and Answers

All communications in regard to editorial business should be addressed **to The EDITOR, British Medical Journal, B.M.A. House, Tavistock Square, W.C.1.**

ORIGINAL ARTICLES and LETTERS forwarded for publication are understood to be offered to the *British Medical Journal* alone unless the contrary be stated. Correspondents who wish notice to be taken of their communications should authenticate them with their names, not necessarily for publication.

Authors desiring REPRINTS of their articles published in the *British Medical Journal* must communicate with the Financial Secretary and Business Manager, British Medical Association House, Tavistock Square, W.C.1, on receipt of proofs. Authors over-seas should indicate on MSS. if reprints are required, as proofs are not sent abroad.

All communications with reference to ADVERTISEMENTS, as well as orders for copies of the *Journal*, should be addressed to the Financial Secretary and Business Manager.

The **TELEPHONE NUMBER** of the British Medical Association and the *British Medical Journal* is EUSTON 2111 (internal exchange, four lines).

The **TELEGRAPHIC ADDRESSES** are:
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MEDICAL SECRETARY, *Mediseca Westcent, London.*

The address of the Irish Office of the British Medical Association is 18, Kildare Street, Dublin (telegrams: *Bacillus, Dublin*; telephone: 62550 Dublin), and of the Scottish Office, 7, Drumsheugh Gardens, Edinburgh (telegrams: *Associate, Edinburgh*; telephone: 24361 Edinburgh).

QUERIES AND ANSWERS

Chenopodium

Dr. L. L. STEELE writes: Can any toxicologist advise me as to the poisonous effects of oil of chenopodium? I gave ♏ xxx to a six-months-old pointer dog on March 19th and, one hour later, ʒi ol. ricini; the dog showed signs of colic, frothed copiously at the mouth, and refused all food. Since then he has been deaf, and, as he is of great value, I am anxious to know if the deafness is likely to be permanent. Otherwise he seems to be quite recovered.

Persistent Nausea in Myocarditis

Dr. S. P. BHATIA (Mexborough) writes: I shall be very grateful if any of your readers can suggest a remedy likely to relieve persistent nausea. The patient has complained of this distressing symptom for the last few months. He has been ill for over a year. His condition has been diagnosed as myocarditis. Resting pulse rate varies from 104 in the morning to 120 in the evening. Blood pressure varies from 175/115 to 210/120 mm Hg. Urinary albumin varies from 1/2 to 2 grams per litre of urine. There are no casts. It has been suggested that this persistent nausea may be due to chronic uraemia; but he has no headache, no abdominal pain, no diarrhoea, has never been actually sick, and there is no dryness of the skin. Disks are normal. Blood non-protein nitrogen is 53 mg. per 100 c.cm. of blood. Blood has been examined for indican with negative result. Rest, fluids, fruits, and vegetables have failed to relieve his distressing and persistent nausea.

Claudication and Raynaud's Disease

Dr. J. FRELICH (London, W.9) writes: Professor John Fraser's address on circulatory diseases of the extremities is of great interest to me as I am attending a patient, aged 52, for intermittent claudication. The *x*-ray report states "there is evidence of early calcification of the right popliteal and posterior tibial arteries." The pain in the leg dates from 1930. The patient tells me that about ten years ago his fingers used to go white, accompanied by a good deal of pain and "pins and needles"; this recurred during the following two years, but not since. I would like to know if other medical men have met with the two conditions—namely, intermittent claudication and Raynaud's disease in one individual.

Lily Rash

Dr. W. H. PALMER (Hayle, Cornwall) writes on the subject of lily rash and its possible prevention by inoculation (see *Lancet*, October 6th, 1934, p. 755): I should be obliged if any of your readers who have been trying a vaccine made from daffodil stalks would let me know: (1) the top dose—that is, number of units reached before picking started; (2) if protection was given to any degree, whether slight or considerable. I am trying to correlate reports from various users. There seems to be no doubt that inoculation confers protection, but the dosage for absolute protection has yet to be determined.

Income Tax

Royalty on Professional Monograph

"X. Z." has arranged for the publication of a monograph. The publishers do not think that the sales will cover expenses, and "X. Z." has to contribute £160 towards the cost of publication, but will have a 10 per cent. royalty on sales. Can he claim to set off the £160 against the royalty receipts?

*** The form of the agreement with the publishers seems to be a little unfortunate, because, prima facie, the £160 is a "capital" contribution towards the cost of originating the royalties, which are "income." On that basis "X. Z." would be liable to account for tax on the latter without any deduction for the former. It is, however, open to question whether, having regard to the fundamental assumption that a loss will be incurred on the publication, the real substance of the transaction is not that "X. Z." has to contribute by way of cover to the publisher a maximum amount of £160, which is reduced by the aggregate amount of the royalties. In equity "X. Z." should have the benefit of any doubt, but we fear that a strict interpretation of the law is against him.

Locumtenent Work

"J. T. M.," who resides in Scotland, did some locumtenent work in England. Can he "deduct an allowance for hotel expenses in between the periods of work"?

*** No. They would not be regarded as having been incurred *in* carrying out the work for which he received payment.

LETTERS, NOTES, ETC.

Anaesthetists' Meeting in Melbourne

A paragraph in the *Journal* of January 26th (p. 164) invited members of the British Medical Association visiting Australia for the Annual Meeting to attend the meeting of the Australian Society of Anaesthetists at Melbourne from September 16th to 18th. The following further information has now been received from the honorary secretary of the society, Dr. Geoffrey Kaye. The official B.M.A. party leaves Melbourne on the morning of September 15th, and the boat will reach Sydney on the evening of the 16th, to leave again for Singapore at noon on the 19th. The meeting of the Australian Society of Anaesthetists lasts from the morning of the 16th to the afternoon of the 18th, but there is a train which leaves Melbourne for Sydney daily at 5.30 p.m., arriving at 10 a.m. next day. It would thus be possible for an anaesthetist who so desired to attend one, two or even three days of the meeting and still reach Sydney before the boat goes. Of course, to miss the connexion at Sydney would be embarrassing, but not beyond remedy. The official boat will doubtless call at Brisbane, which is twenty-one hours by rail or four hours by aeroplane from Sydney.

"For an A1 People Beer is Best"

Dr. H. E. HARGREAVES (Watford) writes: Variations of this advertisement are before our eyes throughout London, urging us to "profit by that knowledge in the campaign to improve the whole nation's physique." In the opinion of many these statements are at variance with actual fact. Beer is an alcoholic drink. I was taught that an increase in the amount of alcohol consumed by individuals making up a nation usually means an increase *pari passu* of tuberculosis, venereal disease, lunacy, crime, and misery generally in that nation. Perhaps those who are in authority will tell us whether this is true or not. Or perhaps this campaign in the interests of beer has the blessing of the Ministry of Health, in which case we shall expect to see vast quantities of beer ordered by that authority for consumption in ante-natal clinics, lying-in hospitals, and infant schools. We should also like to ask medico-legal experts and parliamentarians about the laws governing such propaganda, and what control can be exerted should those in authority deem advertisements to be misleading.

An Emblem for Doctors' Cars?

Dr. EDWARD S. JACKSON (Carnforth) writes: Now that a fixed maximum speed has been prescribed by the authorities for all motorists travelling upon the road through any districts which are classed as populated—whether cities, towns, or villages—would it not be advisable that every medical man who uses his car when called upon to visit urgent and dangerous cases of all kinds should be allowed to wear or carry a small red cross upon the car which would allow him to travel at what speed is necessary for the benefit of his attention to the case without being checked on doing so by the road authorities? I know that such a method of procedure has been adopted in America.

*** We believe that the use of the Red Cross as an emblem by private individuals or firms is forbidden under the Geneva Convention.

Disclaimers

Dr. A. C. FRAZER (London) writes: I would like to state that the report in the *Manchester Guardian* of March 21st on the work done by Dr. V. G. Walsh and myself, on the adsorption of toxins by fine oil in water emulsions, was made entirely without my knowledge, and that the subsequent reports in the lay press were a direct result of this article. I would further like to dissociate myself from any extravagant claims which may have appeared in the newspapers, and to emphasize that work on the adsorption of toxins and the application of this principle to the treatment of pneumonia and other toxaemic conditions is still in progress. This being so, I sincerely hope that the medical profession will defer any inquiries with regard to this matter until such time as the results of these investigations are made public in the proper quarter.

Mr. S. W. DAW (Leeds) writes: My name has appeared in the daily press in connexion with a case treated at the Leeds Infirmary. I wish to emphasize that this use of my name is entirely unauthorized, and that it has been suppressed by me as much as possible.

Mr. P. R. ALLISON (Leeds) writes: It has been brought to my notice that in the *Daily Mirror* for Monday, March 25th, an account was given of a patient who collapsed under an anaesthetic at Leeds Infirmary, and was subsequently revived by cardiac massage and artificial respiration. I find that my name has been associated with this operation, and I wish to state that this was entirely unauthorized. I have received a letter of apology from the Northern Editor of the *Daily Mirror*.

Corrigendum

An error appeared in the *Journal* of March 23rd (p. 614) in the reference to the Ministry of Health's circular relating to Burrow Hill Sanatorium Colony. The age limit for the treatment of tuberculous youths should have been stated as 13 to 19.

Vacancies

Notifications of offices vacant in universities, medical colleges, and of vacant resident and other appointments at hospitals, will be found at pages 41, 42, 43, 44, 45, 48, and 49 of our advertisement columns, and advertisements as to partnerships, assistantships, and locumtenencies at pages 46 and 47.

A short summary of vacant posts notified in the advertisement columns appears in the *Supplement* at page *124*.